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Interactive comment on "Observational evidence of moistening the lowermost stratosphere via isentropic mixing across the subtropical jet" *by* J. Langille et al.

Anonymous Referee #2

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In this manuscript, the authors present the analysis of a double tropopause-intrusion event as a case study to validate the new SHOW instrument. The authors compare their results to reanalysis and satellite data.

I find the work here presented exciting, and no doubt, it lets to get some insight on the potential of the instrument. I want to congratulate the authors for the work developed. At the same time, I have to say that I have detected several mistakes along with the manuscript and that I think that both the analysis and the presentation can be and should be improved. I am familiar with the topic here discussed, and I have found the description sometimes confusing and incomplete. Therefore, those not so familiar with the matter could find it challenging to understand some issues.

One of the more puzzling issues that I have found in the manuscript is that the Introduction is poor in number and the use of appropriate references. There are some striking examples along with the paper because they involve some of the coauthors. This lack of adequate references makes the discussion about the case study not well balanced and can complicate the reader to have a general perspective of the phenomenon. Below I address this issue with some suggestions where it corresponds.

For example, in the first paragraph of the Introduction, it would be appropriate to cite a work that supports the statement on the limitation of models. Sections 6.2.4 and 6.3 of Gettelman et al. (2010) deal with it (https://doi.org/10.1029/2009JD013638). Also, it is usual to cite Gettelman and Forster (2002) when you refer to the CPT (line 37)(https://doi.org/10.2151/jmsj.80.911). The physical mechanisms mentioned (line 54) are well explained with a model and radiosonde data by Ferreira et al. (2015) (https://doi.org/10.1002/qj.2697). It provides an excellent discussion of some of the most relevant constraints, and it would be worthy of citing it to let the reader get some insight on them.

In lines 57-61, the authors discuss the limitations of satellite data. Indeed, they use AURA-MLS for comparison purposes here. I think that they should cite the works validating WV profiles of AURA-MLS for the SPARC Data Initiative, as they provide the background on the validity and limitations of the measurements. At least one of the coauthors of this manuscript is also coauthor of such works:

Toohey et al. (2013) https://doi.org/10.1002/jgrd.50874 Hegglin and Tegtmeier (2017) The SPARC Data Initiative: Assessment of stratospheric trace gas and aerosol climatologies from satellite limb sounders. SPARC Report No. 8, WCRP-05/2017.

Related to the Introduction: the manuscript has two parts, the validation of the instrument and the case study. Therefore, I think that all the information relevant for the case study that lets to interpret the results should be presented first, included in section 1.

In this vein, the current section 6 should be moved earlier in the manuscript, before beginning the analysis and interpretation of the results. Also, the current figure 1 is right; still, I think that it would be good to include a similar isobaric synoptic map (to check the meteorological situation) and the corresponding map for the first lapse rate tropopause. Doing it would let the reader have a broad picture of the situation. Double tropopauses can happen because of several different conditions, and a priori all of them should be had into account. To do it, all this information is relevant. After it, I suggest to include a brief sentence discussing how the region chosen for the ER-2 flight is one of the central global areas of occurrence of double tropopauses including summer as Añel et al. (2008) shows (https://doi.org/10.1029/2007JD009697). The other work cited in the text and typical about the study of double tropopauses, Randel et al. (2007) do not show them for July over the region studied in this work.

In Figure 2, I had to realise that the values of the horizontal axis are different for each subplot. Right now, it is harder to visualise the latitudinal variation and the assessment of the vertical 'peaks', but it is necessary to be able to compare all of them adequately. Therefore, please, use the same axis for every subplot. Also, it would be helpful to contextualise the air masses if you can add a horizontal line at the level of the thermal tropopause (first and second, if possible). In the caption, you have missed the degree symbol before the cardinal points.

Regarding Figure 3, I have several concerns that should be clarified and better discussed:

First of all, it would be useful in you can include the longitude value in the caption. Secondly, the authors do not say how they have computed the thermal tropopause. Did they use its definition (WMO, 1957)?. Was it retrieved from reanalysis?. It has to be clarified. Also, the use of model levels for the reanalysis could have improved the discussion. If you can use them, it would be better.

About the plots: in Fig. 3a the isentropic entrainment in the lowermost stratosphere reaches 40.5 degrees N. However, in this region, the PV values are large (up to 6 PVUs, at least 5 PVUs). No doubt, the WV is of tropospheric origin, but such PV values are much higher than acceptable for tropospheric air. At these latitudes, the larger values expected for tropospheric air are 3.5-4 PVUs. If you check your Fig. 4a it seems clear that the 6 PVUs value that you mention in the text as a value for the tropospheric air, is seen in AURA, not so much in the SHOW measurements (and the AURA measurement fits better with the shape of the potential temperature lapse rate in Fig. 3b). Wang and Polvani (2011) (https://doi.org/10.1029/2010JD015118) and Añel et al. (2012) (http://dx.doi.org/10.1100/2012/191028) have already shown with idealized experiments and lagrangian models how the equatorward movement of air masses trough tropopause breaks at midlatitudes is also very important (and indeed they did it for regions close to the one studied here). Checking the Figure 4b, it could be argued that there is a fingerprint of the movement of stratospheric air equatorward through the break, because of the higher values of ozone that reach the 4 PVUs (near to the more accepted tropopause value) and 36.5 degrees N. Therefore, I think that you need to write the paragraphs from line 204-216 with a more complete discussion and better balance.

A right way of checking the reality of the movement would be with using a lagrangian transport model (as in Añel et al. 2012). If you can include it, it would be a great addition to the manuscript, but I realise that it is not the goal of this work, so I do not consider it a 'must' here. But given that you do not provide it and on the ground of the tasks that I mention above, along with the text, you should relax the language and the level of the statement about poleward isentropic mixing. For example, in line 197, where you say 'it is widely accepted' because there are many different synoptic situations.

Finally, the 'summary and conclusion' section is short. I would include some discussion on the error of SHOW and how it could have impacted the results here presented. Also, I would find it interesting to include a reflection on the limitations of SHOW to sample similar episodes in more poleward latitudes. Given that SHOW seems to have

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a restriction below 13.5 km and that poleward the tropopause height decreases, is SHOW limited to sample these episodes only around the subtropical jet?. In line 304, the terminology of 'tropospheric intrusions' is used again. As said before, I think that talking about 'tropopause breaks' is correct in the context of this work.

References: - The list of references is in the incorrect order. - Randel et al. 2007a and 2007b are not distinguished in the list of references.

Line 28 - de Forster and Shine Line 30 - Gettelman and Sobel, 2000 Line 44 - Appenzeller and Davies, 1992 Line 52 - Pan et al. 2010 is not listed among the references Table 1 - units of speed – km/h (not km/hr) Line 112 - the international units of pressure are hPa, not mb Line 133 - coarser Line 168 - there is not an orange line in the figure Lines 176-178 - this sentence is redundant. It has been discussed earlier in the text. I suggest removing it. Lines 187-189 - in line 188 when you refer to the tropopause, it is hard to know if you refer to the first or the second; please clarify it. Also, it would be useful in you can include in the plot something (an 'A' and a 'B,' a star and a square,...) to make clearer to what region you refer. Lines 248-251: the last sentence is obvious and can undermine the achievements of the SHOW instrument. I recommend to move it to the conclusions as a final reflection. Line 269 - 20 degrees Figure 5 - caption - I would say '230 and 235.5'. I read the plots from top to bottom and the one on the top corresponds to 230. As it is now, it can be not very clear.

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