**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 Received and published: 10 January 2020**

**Overall Author Response:**

Thank you very much for your comments and suggestions regarding our manuscript. We agree with most your suggested changes and have included several edits to the final manuscript that reflect these changes. We believe that the associated changes and additional analysis provides better context for the observations and makes for a more complete paper. We have responded directly to your comments below in red (A.2.#) and have identified where the corresponding changes have been made in the manuscript. In addition to these changes, there have been several format and structural changes that were made to the manuscript in response to suggestions and comments from the first reviewer. Please refer to the responses to the first reviewer for a description of those changes. Note that several new Figures have been added to the manuscript in order to expand the analysis. We have also edited the figures to have the same colormap throughout the paper.

Summary of key revisions made to the paper:

1) A synoptic scale meteorological analysis is included for the Rossby wave breaking event that resulted the observed dynamical structure.
2) Discussion of the process-consistency despite the specific differences between SHOW water vapor structure and the ERA5 dynamical field is made to clarify that multiple factors can contribute to the specific differences, including the physical factor that when wave breaking result in irreversible mixing, the air mass composition loses its correlation with PV as a dynamical tracer.
3) More focused in the objectives and take-home messages of this paper to present the new observational evidence of water vapor transport into lowermost stratosphere driving by Rossby wave breaking and instrument capability and potential impact on stratospheric water vapor budget. Eliminated the additional discussions on the scale of the event and further dynamical analysis to avoid distracting from the main messages.
4) The abstract has also been edited accordingly

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.

## Author Response (A.2.1)

We agree with the reviewer and have revised the introduction to be more thorough in the background work. However, not every work recommended by the reviewer are mentioned. To include all of them, the discussion may become too diffusive. The introduction has been updated in order to provide better context for our case study and ensure the reader has a broader picture of the background and field. This includes edits to the text that incorporate some of the suggested references, as well as, several additional references that we feel helped to contextualize the discussion. These changes have significantly improved the introduction and help to set up the overall goal of the paper.

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.

## Author Response (A.2.2)

We have made structural changes to the paper and further clarified that the objective of the case study is to identify the process of transport revealed by the observation and that the observation further demonstrate the scientific significance of the new measurement capability.

Specific changes:

1) Firstly, we have included a new figure showing 3 - 48-hour time steps (each day at 20:00 UTC) of PV on the 380 K surface for the 6 days leading up to the date of the case study (this is now Figure 2 in the paper). The Figure clearly shows a Rossby wave-breaking event has occurred in the days preceding the flight that results in mixing along the subtropical jet.

2) We have added a new figure (Figure 3) shows the PV on the 380 K (Figure 3 (a)) and 400 K (Figure 3 (b)) surfaces for the 07/21/2017 18:00 UTC time step. In the Figures, the tropospheric and stratospheric air masses are separated by the 6PVU contour on the 380 K surface and 8 pvu on the 400 K surface. Here it is observed that the mixing associated with the Rossby wave breaking results in a long low PV "tongue" consistent with tropospheric air that extends from the Western Pacific and tracks the subtropical jet across North America.

3) To characterize the vertical structure we have included a Figure (now Figure 4) that shows the height of the thermal tropopause and the location/extent and height of the secondary tropopause for the 07/21/2017 18:00 UTC time step. In these figures one can clearly see that there are several double tropopause regions located on the poleward of the subtropical jet. The SHOW measurements track crosses one of these regions. While additional time steps are not shown, it is useful to point out that the regions of double tropopause vary in extent from time-step to time-step. In fact, the double tropopause region that SHOW crosses becomes larger near the 21:00 UTC time step. A paragraph has been included in the text that discusses this Figure. We believe that the updated analysis provides the relevant context for the case study and justifies the suggestion that the moist filament observed along the second tropopause in Figure 6 (a) is likely of tropospheric origin.

4) The goal here is not to validate the SHOW measurements using the reanalysis data but rather show that the measurements are consistent with a mixing event. Therefore, the current section 6 has been removed and the discussion of the spatial extent of the event is now examined in Section 3 with the new Figures showing the full synoptic picture.

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.

## Author Response (A.2.3)

The axes have been adjusted to be the same and we have included horizontal lines noting the altitudes of the first and second tropopause. The degree symbol has been added before the cardinal points in the caption as well as throughout the manuscript where it was missed.

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.

## Author Response (A.2.4)

1. The longitude stayed nearly constant during the flight as mentioned in the text describing Figure 3 and in Section 2. We have added the following line to the caption of the figure: "The longitude is along the 124.5° W line and is nearly constant for the measurements."
2. There is no tropopause product in the ERA-5 reanalysis available to this work. The tropopause we used is derived from the 37 level temperature product. We stated this in the revision and have included a detailed description:

   "Here the tropopause is derived using the ERA-5 temperature field using the lapse rate definition (WMO, 1957; 1992) with a modification. The modified version locates the first tropopause as the lowest level where the lapse rate drops below 2 K/km and remains below that value on average for 1 km (instead of 2 km). A second tropopause is identified if the lapse rate increases above 2K/ km (instead of 3 K/km) and then decreases again below 2 K/km. This is done to remedy the coarse vertical resolution of the of the temperature data. This type of modification has been recognized to allow identification of the double tropopause derived from coarse resolution temperature data that is more consistent with high resolution observational data (Randel et al., 2007). In particular, our goal here is to highlight the spatial extent of the layered static stability structure as discussed in Sections 4-5."

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.

## Author Response (A.2.5)

1) The PV value for representing the tropopause is the topic of Kunz et al., 2011. There the gradient based analysis showed that at 380K the average PV for identifying tropospheric to stratospheric change is 6 pvu. Although not shown it is around 8 pvu at 400K . We include this reference in the revision. We also emphasized in the discussion not the specific PV contour but the weakening of the PV gradient in the region indicates the tropopause break.
2) Yes the discussion focused on the poleward RWB as indicated in the new figures 2-4. For the purpose of this study, the resulting vertical layered structure above the subtropical break is the key. Equator-ward transport is also important but not the focus of this study.

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.

## Author Response (A.2.6)

Indeed we focus the analysis on providing the high resolution measurement and evidence of the transport impact on lowermost stratospheric water vapor and the highlight of new measurement capability. This is clarified in the opening and abstract.

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 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.

## Author Response (A.2.7)

We have updated Section 6 to be an expanded discussion and conclusion section. This section provides discussion on the limitations of the SHOW measurements and how these limitations may have impacted the study is now included in the paper. Specifically, we discuss the choice of the lower altitude cutoff. The lowest altitude cutoff of the measurements is primarily associated the optical depth. At some point, when the optical depth is below 1, scattered light from below is fully absorbed by the atmosphere. This generally occurs a few 3-5 km below the tropopause and varies from profile to profile. Algorithms are in development to actively determine this cutoff during the retrieval process. However, we did not have apriori knowledge of the meteorological picture prior to performing the retrievals. For the current study we chose 13.5 km to fix the altitude at a reasonable height (several km below the expected 15 km -18 km tropopause height for latitudes below the break) that we knew would provide accurate retrievals across the latitude range.

We also reiterate in the discussion section that we are not trying to validate the SHOW measurements with MLS and the reanalysis data. The SHOW measurements provide a much higher spatial sampling compared to either MLS or ERA5 and we are confident in the quoted uncertainties of the SHOW measurements. Therefore, the variability observed in the two-dimensional water vapour distribution observed by SHOW is representative of the true state of the atmosphere. The reanalysis data provides the appropriate meteorological context and the MLS measurements serve to geophysical consistency with the SHOW measurements.

## Minor corrections (A.2.8)

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

- Only Randel et al., 2007a is referenced the paper now

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

- Gettelman and Sobel, 2000 Line 44 - Appenzeller and Davies, 1992 Line 52 - Pan et al. 2010 are no longer referenced in the manuscript
- de Forster and Shine, 1999 and 2000 are included

Table1-unitsofspeed–km/h(notkm/hr)

- Corrected

Line112-the international units of pressure are hPa, not mb

- Corrected

Line 133 - coarser

- Corrected

Line 168 - there is not an orange line in the figure

- Corrected

Lines 176-178 - this sentence is redundant. It has been discussed earlier in the text. I suggest removing it.

- Corrected

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.

- The text has been edited to be less ambiguous

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.

- This statement has been moved to the new discussion section (Section 6)

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.

- Corrected