REPLY TO COMMENTS BY REVIEWER #2 (Dr. S. Fueglistaler)

We are grateful to the thorough reading and constructive comments on our manuscript. We believe we have incorporated all aspects pointed out. The detailed description on the revision follows:

Interactive comment on "Dehydration in the tropical tropopause layer estimated from the water vapor match" by Y. Inai et al.

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General:

Inay et al. present an analysis of in-situ measurements of ozone and water vapour in the TTL over the Western Pacific obtained during the SOWER campaign. They com- pare observations from different locations for cases where they have indications based on trajectory calculations that two stations observe the same air mass, one observation being downstream of the other. Comparison of the two measurements then is used to constrain what has happened to these air masses between the two observations, with a focus on dehydration. The data is very interesting, and the analysis is very carefully done. I am a little surprised that no remote sensing data, e.g. MLS/Aura, is used to better establish the larger-scale structure of the water vapour field during the period of in-situ observations. One may be a bit disappointed that - ultimately - these observations seem insufficient to make a substantial step forward. However, I think this is also an important result, and I recommend publication of this paper. Below, I provide a list of "minor" comments/questions the authors may want to consider for the revised version. Also, I strongly recommend to re-think the organisation of the paper. I do not want to make specific suggestions, but in its present form method description, case studies and more general statements are presented in a way that tends to leave the reader confused.

Reply:
We have re-organized Section 3 significantly. The major changes are described as follows:

Section 3.1 describes conservative property of ozone in the TTL.
The conservative property of ozone in the TTL is a key point for the match analysis. Therefore, it has been described in Section 3.1 based on the first paragraph in Section 3.2 of the original manuscript.

Section 3.2 describes the use of trajectories and the effectiveness.
To explain the methodology, the first to the third paragraphs of the revised manuscript have been rewritten on the basis of statements in Section 3.1 of the original manuscript. However, some statements have been rearranged to explain clearly the methodology (See also next reply).
To confirm whether the methodology using trajectories is effective or not, the second paragraph in Section 3.2 of the original manuscript has been moved to this section with some revisions.

Section 3.3 describes all screening procedures.
To make order of screening procedures clear, a statement "To move on screening procedures for the remaining problems, we use the "conservative property of ozone" as the second principle. Note that these screening procedures are examined after the first step." has been inserted at p.642, l.23.
The description for screening procedure for other nonspecific factors by using consistency of ozone concentration between the first and the second observations has been added to the last part of this section.

The terminology has been re-defined as follows:
"match" is defined as a case that sounding some air parcel more than once
"match radius" is defined as a distance of the criterion for the match
"match circle" is defined as a circle with the match radius

“match circular area” is defined as a region inside the match circle
“match air segment” is defined as a segment included in the both match circular areas of the first and the second observations
“match air parcel” is defined as a cluster of match air segments
“preliminary match” is defined as a case that connected by a trajectories, i.e., match air parcel.
Following these re-definitions, all statements in the manuscript relevant to above have been revised.

To improve the text, the manuscript has been English proofread.

In addition to above, the description for case studies in the original manuscript has been divided into individual description of each case (case 1 - case 4).

Minor comments:
Abstract:
L1-4: Sentence is confusing, be more specific what is poorly understood.
L4: "Match method" - this term is not broadly known.

Reply: The sentence has been changed to “We apply the match technique, whereby the same air mass is observed more than once and such cases are termed a ‘match ‘, to study the dehydration process associated with horizontal advection in the tropical tropopause layer (TTL) over the western Pacific.”

L7: Add information on dataset used for the trajectories.

Reply: A statement “calculated from European Centre for Medium-Range Weather Forecasts (ECMWF) operational analyses.” has been inserted.

L15: Unclear here how the accuracy is determined (i.e. to what does 1-sigma refer - termperature uncertainty, or spread of results for different "matches"?)

Reply: A sentence "The 1σ refers to the spread of results for different matches." has been inserted at the last part Section 4.2.

Text:

P636/L15: I don't think that this absolute statement ("Variations in SWV are poorly understood.") reflects the state of understanding adequately. I think that to leading order variations may be actually even better understood than absolute values (see Fueglistaler et al., 2013; I do not ask to cite this paper, but it lays out the arguments better than what I can do here in this review).

Reply: The sentence has been deleted.

P636/L26: "Cold trap" - there is no definition of what this term is supposed to mean. It refers loosely to the notion that the quasi-stationary temperature field shows a clear zonal structure, but when considering the full space-time varying temperature field, nucelation may occur anywhere, it's just that the probability may be higher in this region. I recommend to not use this term - it has created enough confusion in the community. It is entirely sufficient to say that you have a set of observations in the regions of on average lowest temperatures at tropopause levels.

Reply: The statement has been changed to "The data from SOWER campaigns, together with trajectory calculations, indicate that the dehydration associated with quasi-horizontal advection progresses on isentropes from 360 K to 380 K and that the threshold of homogeneous nucleation corresponding to approximately 1.6 times saturation proved to be consistent with the observations in the altitude region from the 360 to 365 K potential temperature surfaces (Hasebe et al., 2013).".

P645/L13ff: The fact that you find dehydration below 360K is interesting because in general at that level the horizontal temperature gradient in the

region of the observations is not very large, I would think. What can be deduced from the fact that apparently in the layer where gradients should be largest, least dehydration is observed? Are the temperature variations along the isentropic trajectories primarily wave events, or is latitudinal motion important? (I.e. the gradient may be larger in latitudinal than zonal direction?)
For example, when looking at Figure 5b, is the oscillation because of a wave traversing the area, or because of a latitudinal gradient of the isentropes? (Discussion on page 646, line 1-18 does not say much about this.)

Reply: It is primarily caused by wave events rather than the latitudinal motion. A sentence "This SMR variation along the isentropic trajectories is caused mainly by wave events rather than latitudinal motion (as is the case for the other matches)." has been added to the description of Fig. 5.

P649/L18ff: Yes, that's an interesting observation - even more so given that Figure 8 shows actually a local maximum in ozone at 80hPa, which seems not consistent with the explanation provided either (i.e. if injection were higher up, and the local H2O maximum below is due to evaporation of sedimenting condensate, I would expect a local minimum in ozone aloft).

Reply: Is it P648? We pointed a local minimum in ozone at the same level as cold point altitude. Therefore, to make it clear, the statement has been changed to "One possible explanation of the puzzling correlation between water vapour and the ozone profiles is that some convection is injected into an altitude above 380 K where the ozone profile has a local minima at the cold point, after which only ice particles fall to below the 380 K level and evaporate there."

p650/L15: I don't think this is true - the broad general statement would be that convection reaches up to about the level of neutral buoyancy, not the level of zero net radiative heating.

Reply: The statement “the level of zero radiative heating” has been changed to “the level of neutral buoyancy”.

P650/L18: Strictly speaking, the ascent is not "caused" by radiative heating, rather, radiative heating balances the dynamically forced ascent.

Reply: The statement “caused by radiative heating” has been changed to “balancing radiative heating”

P653/L11: Replace "there are little" with "there is little".

Reply: Corrected as suggested.

P655/L15ff: This sentence does not make sense to me - less efficient than what?

Reply: The dehydration is less efficient under the condition that water vapour concentration and temperature are low than that under the condition that water vapour concentration and temperature are high. Therefore, the statement has been changed to “the dehydration associated with horizontal advection becomes less efficient with reduced water vapour and temperature,”.

P656/L1: Boehm et al. is an interesting paper, but reference for sedimentation velocities should be probably the book by Pruppacher and Klett, or a reference therein.

Reply: Revised as suggested. Following this change, the estimated periods to fall through 1 km are updated.
p.656, l.2: 8 hours
p.656, l.7: 20 days

P656/L20: There is a lot of "if we could" in this section here - this section can be shortened.

Reply: Those statements describe something that have to be done for next step of our study.

Figures:
The paper shows a number of scatter plots showing first versus second observation. In addition, the temperature histories along the trajectories are shown for some specific cases. Would it be possible to make a figure that shows the temperature evolution between two points for all matches, with temperature shown relative to that of the first observation? (From a statistical point of view, one might expect temperatures between the two observations to be both higher and lower than at the first observation - but, if I understood correctly - above 360K this seems not to be the case.) It would then also be instructive to show this relative temperature a few days upstream for all matches.

Reply: Unfortunately, we could not make such figure. Instead, the ratios of water vapour amounts measured at the first and the second observation against SMRmin of each match are shown in Fig. 9.

Figure 8: Caption - replace "those" with "the".

Reply: Revised as suggested.

References:
Fueglistaler et al., The relation between atmospheric humidity and temperature trends for stratospheric water, JGR, 118, 1052-1074, doi:10.1002/jgrd.50157, 2013.
Interactive comment on Atmos. Chem. Phys. Discuss., 13, 633, 2013.

Other revisions:

According to the companion paper, temperature bias of ECMWF has been taken into account our SMR estimates. Following this revision, the statement "In this altitude region, we find that the ECMWF temperature has a cold bias of 2 K on the isentropic surfaces ranging from 355 to 360 K (Hasebe et al., 2013). For all subsequent analyses, this bias is taken into account when estimating SMR along the trajectories in this altitude region." has been inserted at p.646, l.24.

Following this revision in addition to the introduction of RHice and RHhom, p.646, l.24-p.647, l.8, "The time evolution of SMR has small perturbations with an SMRmin value of 8.9 ppmv at about 5 hours before the second observation. At this time, the temperature of the air mass is 197.4 K. This SMRmin value is smaller than the water vapour mixing ratio of the first observation. The RHice during advection indicates a maximum value of RHice of 115% with an uncertainty of ±21%. Because the match air mass is dehydrated, this case indicates that ice nucleation must have started before the RHice reached 115%. As this value is much smaller than RHhom, it might correspond to the heterogeneous freezing threshold. A comparison between the second water vapour observation and SMRmin suggests that dehydration continued until RHice reached 60% with an uncertainty of ±16%. If the dehydration does not proceed to less than 100% of RHice, the temperature of the air mass must have decreased by about 3.2 K from the temperature 197.4 K, when the air mass is coldest, falling to 194.2 K on the 356.4 K potential temperature surface."

Right panel of Figure 3 has been re-made because the dashed lines in the original figure were wrong.

Center panel of Figure 3 has been re-made to be reader-friendly.

As a result of quality recheck of sonde data, the number of matches decreased to 107.

Thus, a statement in p.645, l.4-7 has been changed to "Figure 4 shows scatter plots of the first and second observations of the ozone and water vapour mixing ratios for 107 matches (i.e., all of the matches listed in Appendix C). Note that this number includes matches of observational pairs and potential temperature levels. Among the 107 matches there are 25 different observational pairs."

Fig.3 caption l.1: "right panel" has been corrected to "left panel"

Panel (d) of Fig.7: the error bars have been corrected.

Thank you very much again for your valuable comments and suggestions.