REPLY TO COMMENTS BY REVIEWER #1

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:

Reviewer's Comments on acp-2012-915
Dehydration in the TTL from match estimates
by Y. Inai, et al.

The goal of this paper is to estimate dehydration (or hydration) of air parcels advected in the tropical upper troposphere and lower stratosphere using pairs of balloon sonde measurements identified using the trajectory match method. The latter was originally developed for the estimation of ozone loss in polar regions by von der Gathen, Rex and colleagues in the 1990s; this is the first application of this technique that I am aware of to the tropical water vapor dehydration problem. The SOWER datasets used in the paper present potential matches of both Snow White (SW) and cryogenic frost point hygrometer (CFH) water vapor measurements at a number of stations in the greater western tropical Pacific region, and the authors take considerable care to screen out non- representative and otherwise unsuitable matches. As a result the final screened set contains only 110 pairs of observations, and none of the few examples of dehydration above 365 K exceed the uncertainty levels of the water vapor measurements. The lack of a positive result here is disappointing, as this is generally considered to be where the final dehydration of stratospheric air takes place. Below 365 K, significant hydration is found, but the error bars on their results are so broad as to provide relatively little to settle questions on the role of homogeneous nucleation in the UTLS. These disappointments notwithstanding, the authors should be given credit for an honest assessment of the constraints imposed by both the data and the method in this application.
Inasmuch as this paper represents a new approach to an important scientific question and the analysis is carefully done, the paper should be accepted for

publication in ACP. It does, however, need some major revisions. First, the text in Section 3 on the water vapor match methodology should be rearranged in order to more clearly describe the screening sequence. Second, also in Section 3, there is considerable ambiguity in the terminology used to describe the match methodology which needs to be corrected. Third, the text needs careful editing, not only to remove grammatical errors but also to correct improper usage and awkward phrasing that may not be strictly incorrect but do obscure the meaning of the text in certain instances.
Detailed comments with respect to text revisions:

(1) Section 3 together with Appendix A describes the water vapor match methodology and the screening procedures applied to the matched air parcels. The overall methodology is to establish matched observations using trajectories and then to screen out matches according to various criteria. As such, there are problems with the overall organization of this section. Section 3.1 describes the use of the trajectories, but instead of describing the first step in the screening procedure, Section 3.2 jumps to a discussion of how ozone conservation is ascertained, with all of the remaining pieces of the screening bundled together in Section 3.3.

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.

(2) With regard to terminology, Section 3.1 does not adequately define terms related to the matching procedure or the relationships among them, and to a certain extent the reader must deduce for him- or herself what they are, viz.,
・ An “air parcel” is defined by a set of “air segments” defined in lat/lon space, but it does not explicitly state that each air parcel is associated with a specific isentropic layer.

Reply:
To make it clear, a statement “Each air parcel is associated with a specific isentropic layer at every 0.2 K potential temperature level from 350.0 to 360.0 K, and at every 1.0 K level from 360.0 to 400.0 K.” has been inserted at p.639, l.13. Following this revision, p.639, l.23–24 “at every 0.2K potential temperature level from 350.0 to 360.0 K, and at every 1.0 K level from 360.0 to 400.0 K” has been deleted.

· As stated in ¶ 1 of p. 340, there must be both forward and backward matching between the upstream and downstream air parcels to define a "match* air parcel". However, Figure 1 shows a "match air parcel" based on forward trajectories only. I realize that the intent here is to illustrate the difference between air segments that "match" and those that don't, but in doing so the authors have introduced some ambiguity in the meaning of "match air parcel". Indeed, they go on to say that trajectories "as shown in Fig.1" are used to identify "observation pairs", and these are plotted in Figure 2. Are these "observation pairs" the same as "match air parcels"? I would assume so, but it's not clear.

* If the title of the paper had not included this noun, I would have recommended the use of the past participle "matched" which is grammatically correct.

Reply:

To make the explanations for Figure 1 and "match air parcel" understandable, a statement "Figure 1 is an example of a cluster of forward trajectories on the 370 K potential temperature surface calculated from whole air segments gridded inside the match circle centered at Tarawa when the sonde reached the TTL (blue and red dots). Those segments included in the match circular area centered at other observations (also at 50 minutes after the launch) are defined as match air segments (red dots in Fig. 1) and are assumed to constitute an air parcel of the preliminary match (i.e., a match air parcel)." has been inserted at p.639, l.26.

Following this revision, p.639, l.26 "If the air segments advected in the TTL are included in the circular area defined by the match radius centered at other observations (also at 50 min after the launch), they are assumed to constitute a match air parcel." and p.640, l.7 "Figure 1 is an example of a match air parcel identified using forward trajectories." have been deleted.

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.

(3) Errors in grammar and usage include the following:
· Use of the definite article where it's correct to use none at all (e.g. before "deep convection" in line 1 on p. 643; also before "cold trap dehydration" on line 14 on p. 636.)

Reply:
Revised as suggested.
p.643, l.2: penetration of deep convection …
p.648, l.22: is that some convection is …
p.636, l.14: the efficiency of cold trap dehydration.
p.651, l.1: The results showed that cold trap dehydration…

In addition, the manuscript has been English proofread.

· p. 643, l. 3: change "monotonously" to "monotonically"

Reply: Revised as suggested.

· "convection" is a collective noun. It has no plural form (i.e. "convections")

Reply: Revised as suggested.

p.641, l.19: whether convection reached

p.648, l.17: deep convection penetrated

p.660, l.14: some deep convection penetrates

Additional comments on the text:

· Section 3 does not provide the reader with a good sense of the match air parcel population. We discover later on in Section 4 that there were 110 matches that passed all the screening. But how many matches were there in all? How were they distributed between station pairs? How were they distributed in height? Some of this information could have been put in a table. In any, as it now stands, it is difficult to place the three case studies in context. For example, is the match between Tarawa and Mirai at 356.4K shown in Figure 6 the only match that passes the screening tests for that particular pair of sonde launches? I would be assume that the multiple trajectories within ± 5 K of 356.4 K would have provided potential matches for subsequent screening. Did some of these also pass the screening? If so, then are the results similar to the 356.4 K case? If not, then what does this tell us about the robustness of the result for that single match which does? Given that each observational pair has 91 potential match air parcels between 350 and 400 K, this suggests that the vertical coherence of matching is quite low. This would seem to be an important point worthy of comment.

Reply:

Generally, preliminary matches are found at consecutive multiple layers, on the other hand, matches are not like that. It is depending on screening procedures.

According to this comment, we added a list of matches in Appendix.

· In Section 3.2 on p. 641, the upstream-downstream ozone correlation plot in Figure 3 is used to justify the 3-day limit on trajectories used in the match screening. The choice of this particular time limit is not explained or justified, nor are discussed the consequences of relaxing it by one or two days, for example, and thereby allowing more matches through the screening.

Reply: A sentence “This threshold is chosen as the smallest possible time required to obtain the necessary number of samples for the following statistical test.” has been inserted in p.641.

・ In a similar vein, I don’t fully follow the choice of +12 K for the brightness temperature difference criterion for the screening of convective penetration – at least in terms of the graph in Figure A4.

Reply: Because correlation coefficients have some gap between 11 and 12 K of <deltaTbb>min as shown in Fig. A4, we chose this value. In addition, this value seems to be reasonable if it is converted to corresponding geometric height. Therefore, a sentence “This margin corresponds to 2.0-1.2 km in geometric height for a temperature lapse rate of 6-10 K km−1.” has been added to p.643, l.19.

・ Section 4.1 presents two main statistical results: the ratio of the observed water vapor mixing ratio from the sonde upstream to the minimum saturation mixing ratio (SMR) along the trajectory and the same for the downstream sonde. (Calculations here are restricted to the 350-360 K layer, since there were so few examples of either dehydration or hydration above 360 K.) The former value, 207 ± 81% is interpreted as the upper limit on RHice before nucleation. Given the large error bars, this strikes me as not a particularly compelling result, but the mean value itself seems extremely high. How does this high value of RHice compare to the sonde RHice values themselves? It seems to me that this estimate of the maximum RHice along a trajectory should be no greater than the maximum RHice observed at those levels by the sondes in the region in question – unless there were some reason to believe that the sondes themselves are not sampling a full range of atmospheric conditions, an unlikely proposition.

Reply: The main statistical results are updated according to comments from reviewer #3 and Hasebe et al. (2013) (see also “other revisions”), i.e., the values of homogeneous threshold and bias of ECMWF data are taken into account of the estimation. As a result, the estimated value is approximately 146% in RHice, such supersaturations in the TTL are often observed by sonde (e.g., Shibata et al, 2012, JGR, Inai et al., 2012, GRL).

Comments on the figures:

Overall the figures are beautifully drafted with a high complexity of detail. However, for many plots, this forces the reader to zoom in to see the important details, particularly in the numerous scatter plots. Because of this, the paper can really only be read properly online. Granted ACP is an online journal, but I personally prefer to download and print a paper for serious reading – this can't be done in the present instance without spending considerable extra time to blow up the figures individually, and even then, the fine detail suffers.

Unfortunately, we have no idea to make it clear.

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.