Reviewer's Comments on acp-2012-915

Dehydration in the TTL from match estimates

by Y. Inai, *et al.*

Summary:

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.

(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.
- 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.

(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.)
- p. 643, l. 3: change "monotonously" to "monotonically"
- "convection" is a collective noun. It has no plural form (*i.e. "convections")*

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.
- 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

* 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.

consequences of relaxing it by one or two days, for example, and thereby allowing more matches through the screening.

- 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.
- 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 RH_{ice} 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 RH_{ice} compare to the sonde RH_{ice} values themselves? It seems to me that this estimate of the maximum RH_{ice} along a trajectory should be no greater than the maximum RH_{ice} 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.

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.