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Atmospheric Chemistry and Physics Discussions

Interactive comment on "In situ observations of "cold trap" dehydration in the western tropical Pacific" *by* F. Hasebe et al.

F. Hasebe et al.

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We would like to appreciate constructive comments on the manuscript. We have incorporated all aspects raised by the reviewer. The addition of a figure illustrating the relationship between the observed water amount and the saturation mixing ratio of the corresponding air parcel has been extremely fruitful. The detailed description on the revision follows.

General

Major revision is made by eliminating Fig. 2 and by shortening Sect. 2. A new figure (Fig. 7) that shows a scatter plot between the minimum saturation mixing ratio of the core region along the trajectories and the observed mixing ratio on 350, 353, 355 and 360 K surfaces is added. We believe the overall contents of this paper has been

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substantially improved by the discussion associated with this figure (Sect.4).

Following the comments by Reviewer #3, the term 'cold trap dehydration' is no longer used including the title of the paper.

Appreciating the contribution of Y. Inai on the comparison of air parcels' temperature with brightness temperature of geostationary satellite, he is added as a coauthor.

- In section 2.1 (p 6908, l 6-8) the authors state that "trajectory calculations provides useful information on the origin and the water vapor content of air parcels". However, I think that trajectory calculations as presented in this study can only provide information on the minimum SMR, but not on the actual water vapor content of the air masses. Processes like supersaturation, small-scale mixing, diffusion or convection cannot be taken into account, but the large difference between calculated and observed water vapor mixing ratios (factor of 2) indicate the importance of such processes as already mentioned by the authors (p 6916, l 12/13). Therefore I think the results do not necessarily provide an observation of "cold trap" dehydration in the TTL.

> We are also aware of the limitation of the trajectory analysis. However, it does provide useful information on the history of the air parcels exposed during some time before observed by water vapor sondes. Following the suggestion by Reviewer #1, we added a new figure as mentioned above, which we believe better illustrates our findings. Some of the points to be noted here will be 1) the air parcels with lower SMR_min tend to have lower OMR on each isentrope, 2) appreciable number of air parcels go through the cold region without being dehydrated to the level of SMR, and 3) both OMR and SMR_min are mostly higher in Tarawa than in Bandung if compared on the same isentropes.

- The analysis is based on a very small number of observations, 4 soundings over Bandung and 3 soundings over Tarawa. In two cases (Bandung), measurements with the CFH (Cryogenic Frostpoint Hygrometer) and the SW (Snow White) hygrometer are available. First of all, I think that the observational data are too sparse to allow for

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a comprehensive examination of the dehydration mechanism in the TTL. The current analysis should be treated as a “case study”. Furthermore, the first profile over Bandung (08-12-2003) shows large differences between CFH and SW, especially between approximately 150 and 120 hPa. In this region the measurements show even opposite vertical water vapor gradients. In the second profile these differences are less pronounced. However, I think that these differences have to be explained, in particular as the Tarawa soundings use only SW. I would like the authors to add a discussion of the accuracy and uncertainties of the water vapor soundings to show that the measurements are appropriate for this kind of analysis. Differences of 5 ppmv and more cannot be neglected when discussing the efficiency of a “cold trap” dehydration and the importance of other effects like supersaturation.

> We agree that the data shown here should be regarded as a case study. The observational uncertainties of SW in the TTL has been made by Fujiwara et al. (2003) and Voemel et al. (2003), and are referred to in the 1st paragraph of Sect. 3. The vertical profiles are reexamined by considering the response time for the frost-detecting mirror to maintain the frost. The water vapor mixing ratios are derived from smoothed frost point temperature instead of smoothing instantaneous water vapor mixing ratio along the launching sequence. Due to the nonlinear dependence of the water mixing ratio on the frostpoint temperature, we now have much reliable results than before. The spurious oscillations in both CFH and SW water measurements have mostly gone in the lower TTL. The SW data above the level that reached the frostpoint temperature below -80C are omitted (4th paragraph of Sect. 4). We believe the revised profiles better illustrates the actual profiles and fit for an examination of the dehydration efficiency, although we should still be aware of the limitation of the measurements.

- Overall the presented results and conclusions are rather vague. Statements like “the interpretation could be” (p 6913, l 22) or “could be regarded” (p 6915, l 22) show the uncertainty of this study. First of all, I recommend to check the quality and reliability of the water vapor measurements carefully in order to make sure that the discussed

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differences between the soundings are due to different atmospheric conditions and not only due to measurement errors. Furthermore, I would like the authors to re-examine their analysis in order to get a more quantitative estimation of the efficiency of the "cold trap" dehydration. However, I am not sure that the available observations and the applied methodology are appropriate for this kind of analysis. > As mentioned above, all sonde profiles are revised. There is limitations, but the new figure (Fig. 7) and a brief investigation of the IR images have made it possible to discuss the dehydration efficiency in better ways (Sect. 4). The sentence p 6913, l 22, together with others, are rewritten. That of p 6919, l 22 is deleted.

Specific

- p 6904, l 19: is projected

> Corrected.

- p 6904, l 24: Please add a second reference: Forster and Shine, 2002, GLR, 29, 1086.

> Citation is made.

- p 6904, l 25: I miss a hint to the discrepancy between Boulder and HALOE concerning the stratospheric water vapor increase.

> The argument here is a comparison with methane increase. That with HALOE is mentioned in p 6907, l 9. This sentence is modified.

- p 6905, l 6: There are different papers dealing with the impact of water vapor changes on ozone chemistry, e.g. Dvortsov and Solomon, 2001, JGR, 106, 7505-7514; Stenke and Grewe, 2005, ACP, 5, 1257-1272.

> Citations are made.

- p 6905, l 26: Please include the keyword "atmospheric tape recorder".

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> The sentence is modified to include the keyword.

- p 6906, l 8: Which region is meant by “cold trap region”? The western tropical Pacific?

> Holton and Gettelman (2001) obviously presumed the western tropical Pacific as a "cold trap region." The term "in the western tropical Pacific" is added.

- p 6906, l 28: There is a second paper of Dethof et al., 2000, QJRMS, 126, 1771-1788.

> Citation is made.

- p 6907, l 11: There is a current paper on this topic: Randel et al., 2006, JGR, 111 (D12312).

> Citation is made.

- p 6908, l 1-3: I think the discussion in section 4 should not be term as a “new approach” (see below).

> The sentence is modified. It now reads, "The efficiency of dehydration is discussed by comparing the observed water vapor mixing ratio with the saturation mixing ratio of the corresponding air mass estimated from trajectory calculations (Sect. 4)."

- p 6908-6910: Section 2 presents a very general discussion of trajectory calculations. I don’t see the link to the following analysis, so maybe the authors could clarify or shorten this part.

> Section 2 is shortened together with eliminating Fig. 2.

- p 6913, l 2: Please add the Goff-Gratch equation. Both papers are somehow historic and might be hard to get.

> The equation is written down in text. Recent review paper is cited in addition.

- p 6914, l 24: What is meant by the “instantaneous in situ effect”?

> The sentence is modified by avoiding to use this term. It reads, "... could be brought

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about by the dehydration that took place during horizontal advection before arriving at Bandung."

- p 6917: In section 4 the authors discuss the use of a so-called “match technique” to determine atmospheric composition and meteorological conditions along trajectories by repeated radio sonde ascends. This is a very interesting idea. However, since the paper does not present any results of such measurements, I suggest to shorten this paragraph substantially. It is a nice outlook, but I think it is not relevant for the understanding of the present study.

> Revised as suggested by briefly mentioning the term after discussing the results from a newly included figure (Fig. 7).

- The figures are very tiny, at least in the print version. I would like to recommend to enlarge the figures for the final version.

> Figures appear bigger in online version than in print version. Since all figures are provided in postscript files, the typesetter will determine the appropriate size for the final version without technical problem. The modification of Fig. 6 (previously Fig. 7) may help a little.

- Fig. 1, 2, 7: Please add some more tick marks and labels to the SMR colorscale.

> Rewritten as suggested.

- Fig. 1, 7: Black (red) indicates low (high) SMR, but also the location of the air parcel. Especially Fig. 1, upper panel, is a little confusing. Maybe the authors could choose other colors to mark the core and vicinity of the air parcel.

> As the appearance will depend on each printer or computer display, we are not pretty sure if we have done satisfactorily, but the color of the core has been changed to purple while the vicinity and outer-most boundary is shown by yellow-black dotted lines.

- Fig. 2, upper panel: I think it is hard to distinguish between the trajectories in the

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longitude-height section, as the trajectories intersect. Since the SMR is not discussed in the text, I would suggest to use the same color code as in the horizontal plane (blue=380 K, green=370 K, ...).

> Figure 2 is eliminated during the course of shortening of Section 2.

- Fig. 3, 4: The crosses and vertical lines are hard to find. Maybe they should be colored.

> The crosses and vertical lines are now in white following the change to the color version.

- Fig. 5: I think it is not necessary to show the region below 200 hPa.

> Rewritten as suggested.

- Fig. 7: I would prefer to have one graph for each day.

> Rewritten in four separate diagrams in line with the suggestion, though the illustration is limited to representative examples only.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6903, 2006.