Atmos. Chem. Phys. Discuss., 6, S2895–S2899, 2006
www.atmos-chem-phys-discuss.net/6/S2895/2006/

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.

Anonymous Referee #2

Received and published: 4 September 2006

Summary

In the present study, Hasebe et al. discuss the efficiency of the "cold trap" dehydration mechanism in the TTL. The analysis is based on water vapor soundings over the tropical western Pacific (Indonesia and Kiribati) taken during the SOWER campaign in December 2003 and trajectory calculations using ECMWF data. The results show a correspondence between the observed water vapor concentrations and the saturation mixing ratio (SMR) calculated along the backward trajectories, i.e. moist water vapor soundings experienced higher SMR and vice versa. However, there remain large (unexplained) differences between observations and calculations: The observed water vapor concentrations are approximately twice as large as the minimum SMR

calculated along the trajectories.

General comments

Generally I think that dehydration in the TTL is a very interesting topic providing a lot of open questions. However, my opinion is that the current study does not provide substantial new results and insights concerning the dehydration of air parcels in the TTL. Indeed the paper presents a few actual water vapor measurements, but the applied analysis is rather vague. Therefore I believe that the paper needs major revision before it would be suitable for publication in ACP.

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.

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 a comprehensive examination of the dehydration mechanism in the TTL. The current analysis should be treated as a “case study”. Furthermore, the first profile

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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.

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 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.

Specific comments

The manuscript is well written. In case the authors want to submit a revised version, I have some additional, but minor comments:

- p 6904, l 19: is projected
- p 6904, l 24: Please add a second reference: Forster and Shine, 2002, GLR, 29,

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1086.

- p 6904, l 25: I miss a hint to the discrepancy between Boulder and HALOE concerning the stratospheric water vapor increase.
- 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.
- p 6905, l 26: Please include the keyword “atmospheric tape recorder”.
- p 6906, l 8: Which region is meant by “cold trap region”? The western tropical Pacific?
- p 6906, l 28: There is a second paper of Dethof et al., 2000, QJRMS, 126, 1771-1788.
- p 6907, l 11: There is a current paper on this topic: Randel et al., 2006, JGR, 111 (D12312).
- p 6908, l 1-3: I think the discussion in section 4 should not be term as a “new approach” (see below).
- 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.
- p 6913, l 2: Please add the Goff-Gratch equation. Both papers are somehow historic and might be hard to get.
- p 6914, l 24: What is meant by the “instantaneous in situ effect”?

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- 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.

Figures:

- The figures are very tiny, at least in the print version. I would like to recommend to enlarge the figures for the final version.
- Fig. 1, 2, 7: Please add some more tick marks and labels to the SMR colorscale.
- 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.
- Fig. 2, upper panel: I think it is hard to distinguish between the trajectories in the 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, ...).
- Fig. 3, 4: The crosses and vertical lines are hard to find. Maybe they should be colored.
- Fig. 5: I think it is not necessary to show the region below 200 hPa.
- Fig. 7: I would prefer to have one graph for each day.

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