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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

We agree that there are limitations in the accuracy of water vapor observations in the upper troposphere and in the stratosphere. Those shown in the paper are the results of our best effort available, though there are improvements in the instrumentation after December 2003. We also agree that those discussed in the paper should be regarded as a case study; it takes time longer than anticipated, but we are steadily accumulating observational data. We are also aware of the limitation of the trajectory analysis. The statement that observed mixing ratios are roughly twice that expected from minimum saturation mixing ratios along backward trajectories is replaced by a qualitative description. The reliability of the water vapor profiles is improved as detailed below.

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.

Specific

Abstract

- l8: '... are supposed to follow ... ' is a weird phrasing.

$>$ Changed to '... follow'.

- l12: Formulate in relative terms: Temperatures are low everywhere in the TTL, what you want to say is, I assume, that drier air masses experienced lower temperatures than the moist ones.

$>$ Changed to '... drier air parcels were exposed to lower temperatures than were more humid ones during advection.'

- l13-15: As said above, I don't think that you can provide strong support for this factor 2. Hence, you may mention it in the 'Discussion' or so, but not in the abstract.

$>$ The sentence is replace by a qualitative description under the notion that the the number of observations is quite limited.

1 Introduction

- p6906/l17: Perhaps briefly mention the difference of the impact on O3 and H2O if waves break/not break.

> A sentence is inserted. 'In case of wave breaking, irreversible mixing in the upper troposphere leaves it under the influence of ozone rich/dry stratospheric air.'

- p6908/l1-3: See below.

> The mentioning of 'match' is deleted here. The replaced sentence 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).'

- p6909/l1: Please be more specific what kind of product you are using; it appaers you use winds and temperatures on standard pressure levels only (line 27, same page). I am sure you are aware that the model levels are spaced closer, and that you are using a degraded data product? In any case, you should state this clearly in your manuscript. Also, do you use 6-hourly data, or do you make use of the forecast data in between the 6-hourly analysis time step?

> We would like to use higher resolution product in the future study. Currently we use the so-called 'consolidated dataset' that provides data on standard pressure levels with 12-hour time interval. Specification of product is added in text such as, '... operational analysis of 2.5 degree latitude-longitude grid spacing and 15 standard pressure levels with 12-hour time interval.'

- In that same paragraph, you also provide some discussion of trajectories in general, i.e. that they can be very dispersive etc. I am sure you are aware that there exists a large number of publications particularly addressing these issues. Thus, you may want to shorten sections 2.1-2.2.

> Subsections 2.1 and 2.2 are shortened by merging into Section 2 and eliminating Fig. 2.

- It appears that you always use isentropic trajectories, is that correct? If so, please

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add 'isentropic' wherever you discuss them, else it is confusing. Have you considered using the omega field? At the levels where your analysis focusses on (below 360K), convection may be important. Of course, ECMWF may not get the convection correct, but at least such calculations could give a hint on whether isentropic back-trajectories are the correct method, or whether recent convection may have played an important role (see also below).

> Yes, that is correct. The reason of using isentropic trajectories is that the use of vertical wind causes 1) relatively large scatter among nearby trajectories due possibly to spurious omega field, and 2) relatively strong dependency on the choice of analysis field (such as ECMWF and NCEP/NCAR). The term "isentropic" is inserted in the abstract (p6904, l5) and Introduction (p6907, l27). It is also mentioned in the last paragraph of Sect. 2. The effect of the convection is considered by looking at the IR images of geostationary satellite. These points are added in Sect. 4.

3 Analysis of the water vapor sonde data

- p6911/l24: Have you considered tide-effetcs due to solar insolation? (Probably small for the levels you discuss, i.e. 'lower' TTL; but perhaps deserves to be mentioned.)

> Possible tidal effect is ignored. A sentence is inserted.

- p6912/l20ff: Please be a bit more specific which features/pressure levels you discuss.

> The phase line is plotted in Fig.4 (now Fig.3). The sentence is modified. '... such as that shown in purple dashed lines, mutual ...'

- p6913/l13: It is certainly consistent with what you call 'Kelvin wave effect', but note that it is also the standard behaviour of air being above the level of zero radiative heating in the absence of spatio/temporal temperature fluctuations.

> We don't think the air above the level of zero radiative heating is generally saturated. The stratospheric air is obviously not saturated though the radiative heating is positive. Do you mean something different? Anyway, the air could actually be saturated in the

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absence of Kelvin waves. The sentence is modified. '... mentioned above, though other factors may also contribute.'

- p6913/l20-25: This discussion is somewhat too qualitative. At least an attempt of a mixing budget for H2O, O3 and pot. temperature could be made.

$>$ Quantitative argument is very difficult and does not deserve at this moment considering the limitation of observational data. Let me explain about the revision of Figs. 5 and 6 (now Figs. 4 and 5). Fig. 5 (now Fig. 4) is rewritten to include potential temperature profiles and SMRs estimated from ECMWF data. Pressure range is changed following the comment by Reviewer #2. 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 non-linear 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. Those sentences from lines 14 to 25 of p6913 are rewritten.

- p6915/Fig. 7: I suggest that you provide a scatter plot of observed volume mixing ratios (vmr) versus predicted vmr based on minimum smr along backtrajectories. There's no new data in that plot that you have not already shown, but it would bring out better the discussed relationship, namely that drier observations generally also have lower minimum smr's, even though their absolute values may not fully agree.

$>$ Suggested plot is added as a new Fig. 7 in Sect. 4. It shows a scatter plot between the minimum saturation mixing ratio of the core region along the trajectories (SMR_min) and the observed mixing ratio (OMR) on 350, 353, 355 and 360 K surfaces. Referring

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this figure, discussion has been made in Sect. 4. 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.

4 Discussion

- p6916/l3-7: This statement is confusing, given that on p6913/l13 you state that you can see the effect of Kelvin waves. It appears that you discuss different pressure levels, is that correct? If so, please be more verbose to avoid confusion. Alternatively, I suggest that you eliminate all discussion regarding data above about 360K, since you don't do much with that data anyhow. A question remaining, though, is why you don't analyze the few cases where you have CFH data up to the tropopause in the same manner (with backtrajectories) also on tropopause levels?

$>$ Sentences are deleted. Reliable CFH data on 360 K and above were obtained in two soundings. Unfortunately they were almost identical in terms of OMR and SMR_min (Fig. 7).

- p6916/l13-25: As stated above, your analysis is not rigorous enough to allow the statement that observed vmrs are about twice that expected from minimum smrs. You should not only discuss the possibility of convective influence, but actually check data, however flawed e.g. satellite brightness temperatures may be; at least it is an attempt to seriously tackle that problem. Also, you should compare ECMWF temperatures with those that you have from the soundings; of course this is not a comprehensive analysis of ECMWF temperatures, and things might look different away from your soundings, but again it would give at least a hint as to how realistic these (ECMWF) temperatures are. As it stands, it may be also that ECMWF operational data simply has a cold bias in that region.

$>$ The comparison of the air parcels' temperature with satellite brightness temperatures

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is made and the results are discussed in the 3rd paragraph of Sect. 4. Possible bias in the ECMWF data is mentioned in the 1st and the last paragraphs of Sect. 4. No appreciable temperature bias in the ECMWF data was found in Fig. 4, that shows a comparison of ECMWF temperature with sonde data in terms of saturation mixing ratio.

- The small-scale waves not resolved by ECMWF would actually make the problem worse, which you should mention in the discussion of the Jensen and Pfister [2004] paper. Even more important, I think, is that the detailed microphysical calculations in that paper would probably NOT support the idea that observed vmrs can be moister than expected from minimum smr by order of 10ppmv, as you find. This should be discussed.

$>$ The discussion is made in the 3rd paragraph of Sect. 4.

- p6917/l3 ff.: Certainly a 'match-style' analysis would be extremely interesting. However, since you do not actually do it, and also do not really provide an approach to overcome the problems that you mention, I cannot see the relevance of this paragraph; at least it could be shortened.

$>$ The mentioning of 'match' is drastically shortened by referring to the results shown in Fig. 7.

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