Atmos. Chem. Phys. Discuss., 6, S2820–S2823, 2006
www.atmos-chem-phys-discuss.net/6/S2820/2006/

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 #1

Received and published: 31 August 2006

General

Hasebe et al. present a study of dehydration in the TTL based on water vapor measurements combined with trajectory calculations based on ECMWF data. The topic and methodology are certainly appropriate for ACP. However, the results and conclusions the paper presents could be more substantial. This is in part due to limitations of accuracy in the observations, too sparse observations to allow analysis on a statistical basis (the paper discusses essentially only a handful of profiles), and a trajectory analysis that is not as carefully done as it should be. Hence, the statement that observed mixing ratios are roughly twice that expected from minimum saturation mixing ratios (smr) along backtrajectories is problematic, and may be simply an artefact of the analysis (more precisely, of the analyses not done in the paper). As I assume that there are

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

not more observations available (or the authors want to save them for another publication), I recommend that the authors substantially improve the analysis of the available profiles (as discussed below) in order to arrive at more reliable results and conclusions.

Specific comments and suggestions

(p=page;l=line)

Abstract

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

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.

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.

1 Introduction

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

p6908/l1-3: See below.

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?

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

ACPD
6, S2820–S2823, 2006

EGU

large number of publications particularly adressing these issues. Thus, you may want to shorten sections 2.1-2.2.

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

3 Anaylysis ...

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.)

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

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.

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.

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.

4 Discussion

p6916/l3-7: This statement is confusing, given that on p6913/l13 you state that you can

ACPD

6, S2820–S2823, 2006

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 analyse the few cases where you have CFH data up to the tropopause in the same manner (with backtrajectories) also on tropopause levels?

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 serisously 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 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.

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.

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