

## ***Interactive comment on “A statistical analysis of the influence of deep convection on water vapor variability in the tropical upper troposphere” by J. S. Wright et al.***

### **Anonymous Referee #3**

Received and published: 4 March 2009

The authors describe their approach to retrieve information about the variability of upper tropospheric water vapor. By using satellite based measurements as input for a trajectory model PDFs are generated to display links between the evolution and distribution of estimated water vapor content and relative humidity. The manuscript is well written in general and shows innovative ideas. However, although the manuscript describes a very interesting and modern approach that makes use of model calculations as well as of satellite measurements there are, especially in the beginning, several weak points in the presentation that need to be improved before publication.

General comments

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1. The discussion of the IWC on p6 and p7 leaves open questions. I think it is necessary to give some more quantitative information about the estimated deviation of the results due to the appliance of the used IWC relationship, at least some range. Also a more quantitative discussion of the following subject would be appreciated rather than stating that the comparison was "qualtatively similar".
2. The qualitative nature of statements in the discussion of the satellite data base used in the study should be improved or statements should be omitted (see detailed section).
3. Although three different sensors with three different spatio-temporal resolutions are used I could not find an estimation about errors/biases that may arise from interpolation of satellite data to a common grid. On the other hand it is claimed that equation (2) is accurate to within 0.14% (which I do not doubt).
4. How many days with measurements did you use per month to calculate the trajectories? A different number of days per month could introduce a seasonal bias.
5. The matching time frame for "after" is one hour to avoid mixing (p10, l12-23). In the ongoing analysis you use trajectories that extend up to 48 hours in time. The air along these trajectories will surely mix. Is this somehow accounted for? How does mixing influence the results?
6. I am not sure if I understood how you calculated the GRD distribution, could you explain that? Is it only based on observations or do you also use the model for calculating GRD?
7. Please explain TIM. Do you look at a given location and just watch it aging or do you actually follow a point on the trajectory counting its time? Given the wind speed of 9m/s the latter should be pretty much the same as DST, isn't it?

Specific comments

p6, l1: Please provide the exact frequencies.

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p6, l28/p7, l1: How can an error calculation for different frequencies help in this context?

p7, l7: Is there any quantification? If not why is it not necessary?

p7, l19: What resolution is meant? Horizontal/Vertical?

p9, l13: Please define "high quality" or give a reference how AIRS data is categorized.

p11, l5: Do you really mean chaotic in a mathematical sense?

p11, l11-13: Smoothing parameters are mentioned, but in my copy of the manuscript I could not find any basic description nor numbers. A short description instead of a long URL would be helpful.

p13, Eq(4): What is "Pr"?

p16, Eq(5): The longer one observes, the shorter  $\tau_{\text{conv}}$  gets. Does this make sense? Also, I think it has to be made clearer when  $\tau_{\text{conv}}$  is used and when FDC. While reading the text it seemed to me that FDC was used first and then  $\tau_{\text{conv}}$  was invented and used instead without changing the wording.

p22, l1 What temperature is meant here?

p23, l17-29: Since the results regarding  $r_{\text{eff}}$  are of a very qualitative nature (p7 l26-29 and p8/l4-7) and because they seem not to be utterly important for the main conclusion I suggest to shorten or leave out this part.

p24, l28 and p25, l1-4: It is a little puzzling that you first say that you agree to the findings of Ryoo and then weaken the point (p26/l1-7). But only to some extent by saying that ice detrainment has a "measurable" effect to downstream water vapor changes. In the mildest case you are just repeating a statement.

p26, l29 and p27, l1-4: This is a repetition of yourself from the beginning where you talk about the IWC relationship and that you only use one relationship throughout the whole

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tropics. However, repeating the issue does not concretize the error that is associated to it.

P28, I16 What is UTH?

Figures

In Figures 5-7 the captions say that they are the same than Figure 1 respectively, but in Fig. 1a you display actually two quantities, while in the Figs. 5a-7a I can see only one.

Tables

In Table 1 I do not understand why the number of trajectories increases for TIM2 compared to TIM1. Can you explain this? The same is true for DST4 compared to DST3

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4035, 2009.

**ACPD**

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