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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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 #2

Received and published: 15 February 2009

Review of "A statistical analysis of the influence of deep convection on water vapor variability in the tropical upper troposphere" by Wright et al.

This manuscript uses convective rain rate data as a proxy for convection and water vapor and links them with trajectories to generate statistical PDFs of moistening and drying along trajectories linked to convective events. The analysis and data is generally good. There are some confusing aspects that need to be cleared up for this paper to be publishable in ACP however. This paper may be suitable after substantial revisions addressing some of the key areas below.

In general:

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1. There could be more discussion of statistical significance and how it is determined. This is done for the difference of the PDFs, but only seems to be mentioned in the caption to figure 1, not for example in the discussion of table 1. What is one to make of the significance of the values in table 1? Is 7% different than 10%?

2.tau_conv (equation 4) makes no sense to me. The discussion needs to be significantly improved on page 16 or it should just be eliminated. You basically define tau_conv = FDC * tau_obs/tau * tau_life. If tau is the length of the period, then if the other statistics do not change, tau_conv is dependent on the length of sampling? What is the difference between the 'timescale of convection' (tau_conv) and 'tau_life'? The discussion is very confusing and needs to be improved.

Also, are the figures (like figure 4) showing FDC or tau_conv: it is not clear.

3. The use of MODIS data for particle size is confusing. As noted, the MODIS ice effective radius product may have severe problems. The results presented in figure 11 do not make much sense to me: the same effect is seen for large and small particle sizes. This needs better explanation, or I would suggest simply dropping the MODIS data and particle size analysis and discussion. It does not add much (if anything) to the conclusions or significance.

4. I'm unclear how you use figure 6 & 7 to get a quantitative time and distance estimates. Aren't these dependent on the choice of F-Ig and F+Ig thresholds?

5. I worry about the coarseness of the analysis inputs: you are basically using a low resolution model, without a lot of upper tropospheric assimilation data into it. What happens if you use higher resolution analysis data (1x1, more than daily). Say from NASA or ECMWF? What is the UKMO vertical grid? I really worry about the temporal resolution as well, and would like to see some analysis (even if limited) with data at higher temporal and spatial resolution.

6. Can you make use of the vertical information better? Can you do this at different

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levels? You note in several places the unique aspect of these data is vertical resolution, but then you do not use it and pick one level.

Specific comments:

p2,l9: "neither very high nor very low": Not goldilocks here, please be quantitative.

p3,l5: Why do models not account for detrainment evolution? Transport and stratiform condensation, and radiative cooling should be reasonable.

p4. l1: This seems to be a tautology to me: "more complete understanding of convective detrainment & UTH" will help improve "deep convective anvil and UTWV relationships" They say the same thing.

p5, l4: AIRS vertical sensitivity for water vapor is pretty coarse in the Upper Trop.

p7, l7: can you be quantitative too about uncertainties in IWC with different methods?

p8, l17: 6.2e-3 g/kg is 6.2ppmm, or ˜10ppmv? Where does this sensitivity come from?

p10, l18: How do you get one hour variations out of DAILY winds at one local time?

p11,l4: Regardless of normalization, changes at the bottom of the layer dominate: AIRS senses total partial column water vapor, and so more mass at the bottom will be sensed.

p12, l10: This does not really look like 'upward'. Can you convince me statistically? See note about confidence intervals.

p13, l2: the table needs confidence intervals. How does one know what the numbers mean and if they are different?

p13, l27: What is the significance of this? Isn't crossing the 50% threshold for +/-w at 50%RH just noise? What would a sampling of humidity noise look like?

p16, l4: The patterns in figure 4b and c and their un-correlated nature make me suppose that what you are seeing is high variability associated with points that drop in and

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out of the stratosphere: what looks like drying or mositening is really just Stratosphere-Tropopsphere exchange that your coarse time and space sampling is not getting right.

p16,l21: I am not convinced that the assumptions about the observations are correct: AIRS does not uniformly sample at all times. Neither are the trajectories sampled uniformly at all times (only one is chosen).

p21, l7: How is theta calculated? from what?

p22, l7: e-folding times are generally not constant with IWC. Heymsfield and Donner (1990?) parameterized fall speed (a sedimentation time) as proportional to IWC, not constant. More IWC = larger particles = lower e-folding time.

p22, l23: 'significance is low': define.

p24, l14: "neither very high or low" : quantify.

p26, l5: How do you get from these figures to "the amount of ice has a measurable influence on downstream water vapor changes"? Please be more specific here. This whole section has way to many 'generally', 'similar'. Please be more quantitative.

p27, L7: Again, vague "some evidence". I don't see it in Figure 11 if that is what you are referring to, and as noted think the MODIS data should be removed. It doesn't add much

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

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