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9, S1954–S1960, 2009

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.

J. S. Wright et al.

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Thank you for your many constructive comments. We greatly appreciate the time and effort that you invested in reviewing our manuscript.

In response to your general comments:

1. We have calculated and incorporated 95% confidence intervals for the values in Table 1, as well as for the quantities shown in Fig. 2 and Fig. 10.

2. We apologize; our presentation of Eqn. 5 clearly created quite a bit of confusion. FDC is not a component in the calculation of the convective timescale (τ_{conv}): N$_{obs}$ is the number of successfully observed pixels (the denominator in FDC), not the number

of convective pixels (the numerator in FDC). Thus, N_{obs} scales with the length of the analyzed period τ (meaning that τ_{conv} does not get shorter when τ is longer). Meanwhile, N_{grid} is the number of TRMM PR footprints needed to cover the local grid box, and is time-invariant. The lifetime of convection (τ_{life}) is an empirical quantity that represents how often observations would need to take place to successfully observe part of all events. In effect, τ_{conv} is a gridded version of the convective timescale τ_{moist} as defined by Sherwood et al. (2006), which represents the mean length of time between individual convective events encountered by an average air parcel.

We continue to believe that τ_{conv} is a more relevant measure of what is important for upper tropospheric water vapor than FDC: it represents the mean relaxation time between convective events rather than how many convective events occur. However, we feel that the introduction of this quantity deserves a more thorough treatment than we can provide in the context of this work and have removed it from this manuscript.

In the discussion paper, Fig. 4a shows FDC and Fig. 4b shows τ_{conv}. The abbreviation has been added to the caption for Fig. 4a in the revised manuscript.

3. We have deleted the MODIS particle size analysis and related discussion from the manuscript.

4. The quantitative time and distance limits that are derived from the distributions shown in Fig. 6 and Fig. 7 are estimated by tracking the relaxation of the time and distance distributions into the background distribution. The statistical descriptors reported in Table 1 are convenient for illustrating this relaxation; however, the same evolution can be observed by comparing the TIMx and GRD distributions or the DSTx and GRD distributions directly. We have clarified this point in the discussion near the end of Section 4.

5. You are correct that the UKMO reanalysis is not ideal; however, at the beginning of the analysis we had only UKMO and NCEP reanalyses to choose from. We were recommended to use UKMO in the upper troposphere. We are currently in the process

ACPD

9, S1954–S1960, 2009

Interactive Comment

of modifying the trajectory model to use the new GMAO MERRA reanalysis, which is reported at 6 hour resolution on a much finer horizontal and vertical grid and includes radiative and latent heating at 3 hour resolution. Unfortunately, these data are not yet publicly available for the entire analysis period, and have only recently (27 April) become available for any portion of it. The ERA Interim data that is currently available to us does not cover any portion of the analysis period. We will perform a portion of the analysis using MERRA as soon as possible.

6. The primary utility of the AIRS vertical resolution for this analysis is that it enables us to focus directly on the layer of maximum detrainment (300 hPa to 200 hPa), rather than the much deeper upper tropospheric layer (500 hPa to 200 hPa) retrieved by the 6.7 micron channel. Although we could perform a similar analysis at different levels, we feel that this manuscript is most effectively focused by limiting the analysis to the chosen layer.

In response to your specific comments:

p2,l9: This statement has been made quantitative in terms of TRMM FDC.

p3,l5: Many GCMs do not include transport of condensate at all. There is also a great deal of variability among GCMs and relative to observations regarding atmospheric ice water content (see Waliser et al, 2009).

p4,l1: What we are trying to say here is that a better understanding of the physical interactions may help to improve the heavily parameterized representations of these interactions in models. We have edited the statement accordingly.

p5,l4: The vertical sensitivity of AIRS in the upper troposphere is much improved relative to previous instruments, and this improvement allows us to focus on the layer where deep convective detrainment is at a maximum.

p7,l7: We have included a rough quantitative estimate of the uncertainty associated with this equation (about a factor of 2), which is based on applying a range of reason-

ACPD

9, S1954–S1960, 2009

Interactive Comment

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Interactive Discussion

Discussion Paper

able mass-dimension relationships. Deierling et al. (2008) showed a graphical analysis of ten other Z_e-IWC relationships, and our Eq. 1 falls nearly in the middle. For reference, see Fig. 6 of Deierling et al. (2008); Eq. 1 lines up very well with the curve corresponding to Atlas et al. (1995).

p8,l17: Thank you for pointing this out. We have added a citation to Gettelman et al. (2006)

p10,l18: We do not require one hour variations in the wind – we are focusing on the one hour after the trajectory is predicted to be at a given point, within a volume ($1^\circ \times 1^\circ \times 50$ hPa) that will encompass many potential errors in trajectory position. The time resolution of the trajectory model is less than 30 minutes, and the data are interpolated at each time step. As mentioned above, we are currently working to incorporate a higher resolution reanalysis; however, given the sensitivity that we have found in other trajectory-based studies with large sample sizes (e.g., substituting UKMO with NCEP or vice versa), we do not expect a substantial difference in the results.

p11,l4: Your point is well taken; however, we are referring here to the two sub-layers within the analysis layer (i.e., 300 hPa to 250 hPa and 250 hPa to 200 hPa). The normalization enforces roughly equivalent weights for each sublayer in the calculated distributions (rather than allowing the lower sublayer to dominate). We have clarified this point in the text.

p12,l10: The LNK distribution is shifted upward in each RH bin relative to the GRD distribution, down to about 20% RH. We have clarified this in the text.

p13,l2: We have added 95% confidence intervals to Table 1.

p13,l27: The significance can be seen by comparing it to the GRD distribution. If water vapor changes in the tropical upper troposphere were dominated purely by horizontal mixing, then a threshold at 50% would not be particularly compelling. The dominant dynamical influence is subsidence, however, and a threshold at 50% is very different

ACPD

9, S1954–S1960, 2009

than a threshold at 25% (as seen with respect to the GRD distribution). We have also calculated a distribution of humidity noise (we multiplied a uniformly distributed random number by the saturation mass mixing ratio for each scene), and it looks nothing like the distributions presented in the manuscript. There is a maximum along the RH axis near the middle of the range (50% to 60% RH), and an essentially equal probability of the normalized change in water vapor. F_{+lg} is about 70% and F_{-lg} is about 10%, leaving about 20% for the moderate changes that comprise the bulk of the presented distributions. The large probability of very large moistenings at low RH and the use of the Gaussian kernels renders it impossible to accurately characterize the threshold RH between mostly moistening and mostly drying (as defined for Table 1) for this distribution, but it is much higher than the 50% RH threshold calculated for LNK.

p16,l4: We agree with your assessment outside of the deep tropics, and stated as much in the text (p.4051, l.5-13). We have revised this portion of the text to make it clear that we are talking about stratosphere-troposphere exchange. In the deep tropics, we contend that the uncorrelated nature has more to do with the denominator of the ratio; that is, the large changes due to convection seen in the LNK F_{+lg} get washed out in the GRD F_{+lg} because the preponderance of GRD scenes in these locations are not directly related to convection. This is the relevance of τ_{conv} as plotted in Fig. 4b and the number of trajectory points analyzed in each location as plotted in Fig. 4c. F_{+lg} is generally larger for both GRD and LNK scenes when the convective remoistening timescale is intermediate (i.e., convection occurs, but not particularly often). This intermediate remoistening time (which is actually fairly long by tropical standards) implies that the number of local trajectory points will be lower, thus reducing the GRD F_{+lg} relative to LNK even though the LNK cases are included in GRD.

p16,l21: Only the TRMM observations are included in the calculation of this quantity; neither the trajectories nor AIRS is relevant. The TRMM PR observations do meet these criteria.

p21,l7: Theta is calculated from UKMO data at the initialization point of each trajectory.

ACPD

9, S1954–S1960, 2009

Interactive Comment

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Interactive Discussion

Discussion Paper

The text has been amended to include this.

p22,l7: Thank you for the reference. You are correct that the assumption is not strictly valid; however, the standard deviation of estimated mean sedimentation velocities for the set of IWCs that we consider (0.24 m s^{-1}) is approximately one third of the uncertainty associated with the equation used to derive them (0.64 m s^{-1}). The assumption is reasonable within the context of the stated uncertainties.

p22,l23: The fractions of moistening for IWC1 and IWC4 at RH > 90% are different with 60% to 80% confidence, depending upon the RH bin. The text has been revised to include these numbers.

p24,l14: The text has been made quantitative in terms of FDC.

p26,l5: Thank you for this criticism. We have added a figure that elaborates on this. In particular, between 20% RH and 90% RH, the ratio of the differences of predicted Δw IWC4-IWC1:IWC1-GRD is between 0.3 and 0.5. In other words, the most intense convection (IWC$\sim$5 g m^{-3}) contributes an additional 30% to 50% water vapor over the moistening due to moderate deep convection (IWC$\sim$1 g m^{-3}) relative to the background state. We have supplemented this with a similar analysis of the frequency of moistening, and have been more complete and quantitative in the text.

p27,l7: The discussion of particle size has been removed.

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