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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.

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

Responses to general comments:

1. Thank you for these suggestions. We have attempted to revise the manuscript to place it into better context with expectations, as well as to better clarify our rationales for choosing the analysis method and parameters.

You are correct that 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 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 vertical initialization of the trajectories is done at the UKMO potential temperature associated with the geometric altitude of the TRMM observation.

The radiative heating rates are documented in Rosenfield et al (1994). These include UKMO temperatures as the primary input. They incorporate a broadband parameterization of infrared heating and cooling along with a solar absorption routine, and include absorption and emission by carbon dioxide, ozone, and water vapor. We have expanded our description of this in the manuscript.

2. Our findings do contradict this (assuming that we understand you correctly). In revisions, we have added another figure which provides a more quantitative assessment of the role of IWC. 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. In both cases, additional IWC at the convective source leads to additional moistening downstream, and this is statistically significant at 95% through most of the relevant ambient RH range. This means that greater IWC should be related to the parameterized amount of detrained ice, which should then be allowed to advect and exert an influence on upper tropospheric water vapor (this being another serious limitation of many current climate models).

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3. It is possible, but it rarely occurs. Each event is treated independently when we calculate the distributions.

Responses to specific comments:

p4044,l19: This was our intention with the first two paragraphs regarding Fig. 1. We have expanded and (hopefully) clarified these.

p4045,l15: Yes, thank you.

p4047,l9: Yes, one does see the reverse event as well, but the magnitude is not quite so compelling: the water vapor amount can increase by several hundred percent when the air is initially very dry, but clearly cannot dry by more than one hundred percent. These cases are also included in the distribution.

p4048,l14: We're not sure what you are referring to here. Did you mean l4? l10? We could give examples of locations where either of these (suppression of supersaturation and convection influencing very dry locations, respectively) take place.

p4052,l28: Yes, thank you.

p4053,l17: Both of the above. The text has been amended to clarify.

p4053,l23: We have removed discussion of particle size from this manuscript, beyond the occasional nod to other scientists' arguments. The one that you articulate has been proposed by several researchers (e.g., Sherwood 2002).

p4056,l25: Yes, thank you.

p4057,l23: Yes and no. While we would not pin the entire analysis on such bins, we can be reasonably confident that such potential temperatures are very close to the same. We feel that it is thus reasonable to use as an illustration that the relationship between initial IWC and downstream water vapor is not a temperature artifact.

Fig. 1: Yes. We have added this to the caption.

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Fig. 3: We feel that the two methods are interchangeable. We prefer the current presentation because we feel that it best shows the progression between bins, and because it provides a local point of comparison for each set of anomalies. Traditional anomalies would likely need to be calculated with respect to the LNK distribution (since that would provide a referencable base), which would then require the reader to continually refer back to Fig. 1.

References

Rosenfield, J. E., Newman, P. A., and Schoeberl, M. R.: Computations of diabatic descent in the stratospheric polar vortex, J. Geophys. Res., 99, 166778211;16689, 1994.

Sherwood, S. C.: A microphysical connection among biomass burning, cumulus clouds, and stratospheric moisture, Science, 295, 12728211;1275, 2002.

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