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Atmospheric Chemistry and Physics Discussions

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 #1

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Wright et al. present a study of upper tropospheric water vapor in the tropics. They use data from TRMM and AIRS in conjunction with air trajectory calculations based on UKMO winds to analyze the impact of convection on water vapor. Additionally, data from MODIS is used to characterize particle sizes. The topic of this paper is of high relevance to the climate community, and the paper provides interesting insights to the problem from a methodologically new perspective. I recommend publication in ACP subject to the following requests for clarification and modification.

General comments:

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Interactive Discussion

Discussion Paper

(1) Method. I like the authors' decision to perform an analysis whereby the data is grouped according to "event" or "process" type (in this case whether convection has occurred). This is clearly a step forward from more traditional approaches that tend to "group" data based on position/time in a conventional Eulerien framework. However, the downside is that the metrics used are not well established. I had to read the paper several times, and although most information is there, I think the paper would improve if in case of doubt the authors decide for being more verbose, and explicitly repeat the reason(s) why specific results are in agreement (or not) with expectations. As it stands, I could imagine that a reader not thoroughly familiar with this type of approach is quickly lost; and may find it difficult to appraise the results presented in this paper. As a suggestion, an additional figure (the first one) may be used to document the approach for a single case.

One point I got stuck with for a while is that the method chosen in principle could relate three air masses that in reality are entirely unrelated (one before passage of the "convective trajectory", then the convective trajectory, and finally again some entirely new air mass). I can see that the authors try to minimize this possibility by limiting the second observation to be within one hour of passage of the trajectory (if I understood correctly); but the rationale for chosing one hour (and averaging over a day prior to passage) could be explained better. (For example, one could point out that one is not interested in filamental structures, but operates on the assumption that the air mass affected by the convective detrainment has a certain dimension, which then in conjunction with typical wind speeds would explain the chosen timescale of one hour.)

Also, trajectory calculations based on UKMO winds, with one time per day on a relatively coarse grid may not be exactly state of the art. (Also missing is the vertical resolution of this data set, is it on standard pressure levels only?) The initialization of the trajectories is also not quite clear; perhaps I've missed it somewhere in the description on p1440-1441 and p1443; but do you use the TRMM information to initiate the trajectories in the vertical? (I can see only the information "The locations of TRMM ...

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are used as inputs to the Goddard ...", but no statement at which level you initiate.)

Finally, it would be good to have documentation of the calculation of the radiative heating rates. Does this calculation take the full time-evolving 3-dimensional structure of temperature and water vapor into account? If so, where does the information come from? (The manuscript mentions at some point that the layering of water vapor can have important implications for radiative heating rates, but it is not clear whether this effect is considered in this work; or whether it may actually be not important for the aspects studied here for the simple reason that over the timescales studied here, the change in vertical position would be too small to have an impact.) Perhaps even more important is the question whether clear-sky calculations are sensible for this approach: after all, the idea is to trace an air masses expected to be loaded with ice, and it is well established that the presence of condensate strongly modulates radiative heating rates.

Overall, I think the chosen approach and data sets probably deliver meaningful results despite the problems listed above; however, the paper should be more elaborate and convincing as to why this may be so.

(2) The discussion of moistening, and the relation between ice water content and particle size, would probably benefit from some restructuring. In particular, the manuscript should work out better what models do (if I understand correctly, the parameterisations assume more IWC equals larger particles, and hence faster gravitational removal, which could lead to less net moistening in the case that the amount of detrained ice is not the limiting factor.). It is then not quite clear if your findings agree or contradict this ...

(3) Double counts/statistics: Is it possible that your statistics count the same event twice - say in grid box A one hour after detrainment, and in the neighbouring grid box 12 hours after detrainment? If so - do you treat each event as independent when you do the statistics? (I am not sure, but probably that's the correct way to do it for the

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metrics that you analyse; but it may be helpful to be more verbose on this aspect.)

Specific comments:

P4044/L19: As stated above, an example may be helpful.

P4045/L15: First use of "LNK" - does "LNK" stand for "linked"? If so, perhaps say so explicitly, the reader will then remember better what it stands for (ditto for GRD).

P4047/L9: Do you think that in this case a moister air mass mixed with a drier one? But would one not statistically see the same frequency of reverse events?

P4048/L14: Can you give us an indication where (location) this type of event occurs?

P4049/L10: I do not have the TRMM data as e.g. published by Zipser and colleagues readily at hand; but have you had a look at the Bay on Bengal, which may be an outlier to this general maritime vs. continental convection pattern? (I understand that you truncate at 15S/15N, but this case would be interesting.)

P4052/L28: Not quite sure if I understand you correctly, i.e. how can a dry intrusion "counteract" the previous convective moistening? Do you refer to what one observes locally - first an increase in humidity, and then a decrease? What you probably do not want to imply is that the dry intrusion would somehow decrease the net increase in moisture due to evaporation of ice, do you?

P4053/L17: Could you be a bit more precise with what you mean by "detrains larger quantities of ice"? To leading order, I'd think that the rainfall rate and detrained ice should be correlated, is it this you are hinting at? Or is your reasoning more that in the case of continental convection updrafts are faster, and hence more ice is lifted to higher levels, such that the ice detrainment profile would have a larger fraction at higher altitudes than in the case of the maritime counterpart?

P4053/L23: The argument (smaller particles = smaller fall velocity etc.) certainly is

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sensible, but I am not quite convinced whether other effects - such as the details of the initial ambient RH-profile - may tend to have a much larger effect. Or put another way - the argument implies that the amount of evaporating condensate is limited by the gravitational removal timescale. The subsequent interpretation seems to make sense, but the fact that you observe a clearer signal at longer lag times not necessarily increases my confidence in the interpretation. (For example, if there were regional differences in mixing before the convective air mass passes the grid cell, there might be a similar effect?)

P4056/L25: What is "stronger anvil decay"; if you want to refer to the timescale of decay, perhaps "faster" may be the better word?

P4057/L23: Is a binning with bins of 0.1K potential temperature sensible? I am fully aware that this seemingly small pot. temperature difference in the region of interest - i.e. where the lapse rate follows closely a moist adiabate - may represent a reasonable height difference, but the accuracy of the temperature profiles probably limits the usefulness of such fine bins.

Figure 1: Is this the average over all months considered?

Figure 3: Would not conventional anomalies be simpler to interprete (i.e. month minus average over all months considered)? (Question applies also to other figures which were similarly prepared; perhaps there is a reason for the chosen approach that I've missed?)

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

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