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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Correlation among cirrus ice content, water vapor and temperature in the TTL as observed by CALIPSO and Aura/MLS" *by* T. Flury et al.

Anonymous Referee #1

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Review of Flury et al, "Correlation among cirrus ice content, water vapor and temperature in the TTL as observed by CALIPSO and Aura/MLS"

General: The paper presents an analysis of satellite data and reanalysis temperatures to investigate the correlations between water vapor, ice water content of clouds, and temperatures at 100 hPa in the inner tropics. The water vapor analysis includes higher latitudes and pressure levels in the lower stratosphere, but the bulk of the work is focused on the TTL.

This is an important and timely topic. The overall methodology appears sound and most of the conclusions are supported by the analysis. The paper is well written and

the figures are clear.

There are some minor issues with interpretation, however, both for the satellite data and for the NCEP temperatures. I suggest that the following key points should be addressed in order help support the validity of results:

1. Time sampling and diurnal effects. There are known diurnal patterns in tropical convection with significant land/ocean differences. The satellite data are obtained with sun-synchronous orbits and this could have a significant impact on the interpretation. For temperatures, it was not clear whether the daily values were synched with the satellite overpasses or whether these were diurnal averages, and if so, how they were computed. The paper should address the expected impact of the cloud and water vapor measurement sampling (same time at every latitude, throughout the year) and whether there may be any biases introduced and how large these might be.

2. NCEP temperatures. There is the obvious question of why NCEP temperatures are used when MLS has measurements simultaneous with water vapor. If NCEP is preferred for good reasons, then it is important to include some discussion and details on the accuracy, etc. and whether it is sufficient for the purposes of this investigation or whether there are any known systematic or seasonally varying biases.

Minor clarifications, typos, etc.

1. Abstract, first sentence: "Water vapor....has a significant radiative cooling effect..." H2O is a major greenhouse gas, but this reads like H2O has a cooling effect on climate.

2. Abstract, third sentence: This is a statement of results, but there is no previous mention of the methodology so the reader cannot be sure if this is model or measurement based. The first sentence of the conclusion "We calculated the correlation..." is an excellent example of what's needed early in the abstract to make this clear.

3. Abstract, line 9: "...find that the high *anti*correlation occurs..."

4. Abstract, line 10: "...is also highly anticorrelated *with* water vapor..."

5. p.2, near bottom: "..and it is still under debate how water vapor reaches the stratosphere" is contradicted by the next sentence "It is widely accepted that water vapor enters the stratosphere through the TTL." I think what is mean in the first sentence is "the processes that regulate the amount of water vapor entering the stratosphere are still under debate"

6. p.3, near middle: "The ascent of TTL air is predominantly regulated by cirrus clouds ..." It is not clear that cirrus heating necessarily dominates the mass flux. There is net radiative heating and ascent of air in clear regions as well. What is needed is a precise accounting of the mass flux in the cloudy versus clear regions, and a weighting by area in order to assess the relative contributions. Has such an analysis been done?

7. p.5 last paragraph: "The time lag is between 1 and 21 weeks..." A range between 1 and 21 weeks seems rather large for a phase lag between 16.4 and 20.1 km altitude. A mean ascent rate of 0.3 mm/s would be a mean phase lag of 20 weeks over a 3.7-km range. Thus, 1 week seems too short.

8. p.5, bottom: "...which means that the transport toward the southern hemisphere is slower." Since this is based on a mean phase lag for over six years of data, the transport to the SH is slower only in the time mean (there may well be months or seasons where transport to the SH is faster than for the NH). I suggest adding a qualifier "time-mean transport"

9. p6, top: "It is interesting to see...." The intent of this sentence is unclear. The lack of correlation of 121 hPa water vapor with 100 hPa temperature does not necessarily mean that tropospheric processes (such as convection, and radiative exchange with the lower atmosphere and surface, etc) do not have an influence on the thermodynamic state at 100 hPa.

10. p.6 near bottom: ""...and supposed to be the region..." suggest "is thought to be" or "is expected to be" in place of "supposed"

11. p.7 near top: The explanation for why H2O and cirrus are positively correlated in the monsoon regions over Asia and Central America is not clear to me. First, is there abundant evidence for a persistent, elevated tropopause preferentially over these regions during JJA? There could be more discussion and references to help flesh this out. Second, how would this translate into a shift from an anticorrelation to a positive correlation? The details are not obvious to me.

12. p.7 near bottom: Why use CALIPSO IWC instead of MLS? It would seem to be much more straightforward to use retrieved parameters from the same instrument, with simultaneous, co-located measurements. This is similar to one of the questions above concerning the use of NCEP instead of MLS temperature.

13. p.8 near top: The statements about why the correlation is positive in the subtropical NH are repeated here, but I still do not understand them.

14. p. 9 near bottom: "icrease"

15. p.9 last sentence: I suspect what is meant here is that if total water in the TTL is approximately constant in the tropical, seasonal mean, then the mean flux of water into the TTL from below is approximately constant. I think this is correct only provided that the mean transport of water *out* of the TTL and into the lower stratosphere is also constant. Also, this sentence seems to imply that convection does not have a strong seasonal cycle, which is not exactly true. Moist convection refers to many things, and the vertical transport of total water is only one diagnostic.

16. p.10 near bottom, and Fig. 7: This is an interesting but highly speculative explanation. I believe it merits some discussion, but it raised a few questions that should be examined before appearing in the paper. In particular, it seems to rely on the assumption that the maximum cirrus occurrence necessarily occurs over the center of convective regions. Do we know this is true, and if so, why? For example, one could imagine a climatological maximum in cirrus occurring a few hundred or thousand km downwind of convective centers. Something like this could be easily tested using pre-

cipitation data. Secondly, is there a climatological temperature gradient to support this? There should be abundant satellite and radiosonde data available to look for differences in temperature that support the explanation that temperatures in the main convective areas are too low to evaporate all of the detrained ice, compared with the outflow regions. In summary, I believe this discussion should be supported, at least qualitatively, with some observations of precipitation and TTL temperatures.

17. p.12 near top: "...which brings a lot of H2O...." Might be more quantitative here

18. p.12 middle: Statement that convection has no seasonal cycle should be revised (see #14 above).

19. p.12 second to last paragraph: discussion of models seems out of place here since no model results are shown and there is very little discussion previously in the paper.

20. p. 12 last paragraph: Could use more expanded discussion if retained (see #15 above).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25037, 2011.