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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The role of tropical deep convective clouds on temperature, water vapor, and dehydration in the tropical tropopause layer (TTL)" *by* J. H. Chae et al.

Anonymous Referee #2

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This paper is an analysis of MLS data with the goal of using it to gain insight into TTL processes — in particular dehydration of water entering the stratosphere. There is a lot of interesting data presented in the paper, but much of the interpretation of the data didn't make sense or was otherwise problematic. I think this paper is ultimately publishable, but not without major revisions.

Before I get to the scientific problems, I should also note that the paper has major grammatical problems. Before this paper can be published, the authors need to do a thorough rework of the writing.

Major scientific concerns: I am very concerned about the use of MLS data here. As

the authors point out, the vertical resolution of the data is 3-4 km, and this means it may be hard to determine if the MLS measurements are "cloud free" versus "cloudy". Simply comparing the MLS tangent height with the Calipso cloud top height does not seem right to me. The authors need to thoroughly address/describe how the vertical resolution affects their analysis, and how they have addressed this issue.

Several times it is mentioned that clouds in the TTL are associated with convection (e.g., line 17 of page 8969; line 10 of page 8975). However, it seems to me that there is no way to determine if the thin clouds were formed in situ or from convection. Thus, I question much of the interpretation in the paper where the difference between cloudy and clear skies is taken as a proxy for the impact of convection. Given how central this point is to the paper, the authors really need to address this point with great care.

A lot of the data in a paper don't make sense. For example, the negative temperature anomalies in the convective cloud, and positive temperature anomalies above the cloud, have me scratching my head. I was particularly troubled by the statement on page 8970 that the authors' view was that the MLS data from inside the clouds were not reliable. This is a problem. If, in the authors' judgment, the data are not right, don't include them. By the end of the paper, I had no idea what data I should believe.

I was also surprised that there was not more discussion of the previous work on temperature anomalies above the clouds. My understanding of Sherwood's previous work on this was that convection caused cooling above clouds because of detrainment and mixing of very cold air (presumably in the TTL). Given that Sherwood is an author, I disappointed not to see the results of this paper put into context of the previous work that's been done on this. I found the discussion around line 20 on page 8971 about this fully unsatisfactory and not believable.

The authors imply that any data with greater then 100% relative humidity is supersaturated. However, my understanding is that the MLS data have relatively poor precision, and would therefore produce measurements of relative humidities above 100% even if the actual relative humidity was 100%. Thus, I don't know if I believe the statements (e.g., line 23 on page 8972) that the data show supersaturation.

Is it possible that the reason water vapor is lower in the presence of clouds above 16 km is because the clouds are forming by in situ condensation? I see no way to eliminate that as a possibility. If so, then the major conclusions of the paper are really unjustified.

I am particularly troubled by the discussion around line 23 on page 8973 that suggests that air above 16 km is always supersaturated. I don't think that that can possibly be true, and I am not convinced by any of the data shown in this paper.

In the statement at the bottom of page 8974 that descending air at the tropopause will bring down higher mixing ratios is not always true. As the tape recorder demonstrates, during the summer the minimum water vapor is found well above the tropopause, so that descending air will reduce the mixing ratio.

By the end of the paper, the case in favor of the pedagogical model the authors are putting forth was completely undermined by issues with the data, the analysis approach, and the assumptions that went into the analysis. Given that, I don't think this is publishable yet.

Minor comments: The paper is quite long. I would work to try to shorten it. e.g., I don't think they need to have Figure 2 and the associated discussion on page 8968. I would eliminate this.

They should experiment with some different color scales in figures that plot positive and negative quantities (e.g., Fig. 5). It is difficult to figure out where the values switch from positive to negative — I recommend they consider a red/blue color scale.

In equation 1, I think the dq/dz term should be dA/dz. If not, them I'm confused.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 8963, 2010.