Referee #2

We thank the reviewer for his or her comments. The comments help us to seek deeply if MLS data used in this paper is reliable or not, and improve this paper a lot.

1. 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.

We choose MLS cloud free height only if MLS tangent height is 1.5 km higher than CALIPSO cloud top because cloud free feature is dominant in this case. We also convinced it with that temperature behavior above cloud top are still existed up to several kilometer where are definitely cloud free level.

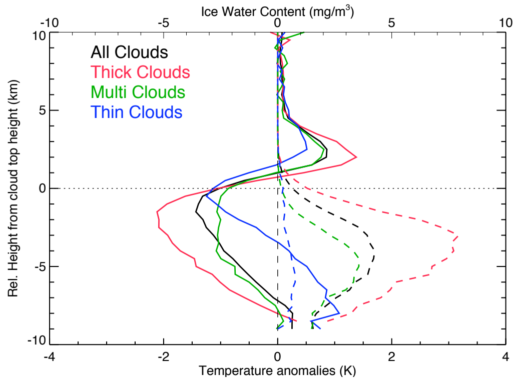
2. Several times it is mentioned that clouds in the TTL are associated with convection (e.g., line 17 of page 8969; line 10 of page 8975). However, it seems to me that there is no way to determine if the thin clouds were formed in situ or from convection. Thus, I question much of the interpretation in 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.

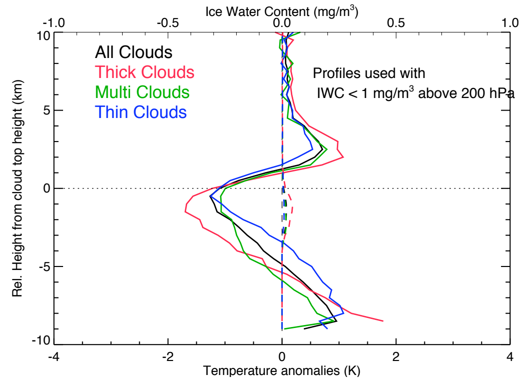
You are correct. It could be my fault if I mentioned cirrus clouds are always generated by deep convection. I agree that clouds in the TTL can be generated in situ far from deep convective area. I added discussion about in situ cirrus in conclusion and disccusion section. However, recent analysis from CALIPSO/CloudSat by Sassen et al. (2009) shows that tropical cirrus is generally tied to deep convection. I do not distinguish between in-situ cirrus and convective generated cirrus in this paper. My key point is in this paper how the existence of cloud in the TTL changes environment variables, and therefore I compare cloudy with clear condition at the same space and height.

3. 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 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.

Temperature distortion during retrieval process (averaging kernel) of MLS might affect on temperature anomalies in deep convective case (but not in cirrus clouds). To reduce temperature error inside clouds, we used uppermost layer of clouds down to 3 km from CALIPSO cloud top in figure 3 (previous figure 4) where is near cloud top height in previous satellite like MODIS and AIRS (e.g., Weisz et al., 2007). The maximum cooling occur 1-2 km below CALIPSO cloud top height as shown in figure 4.

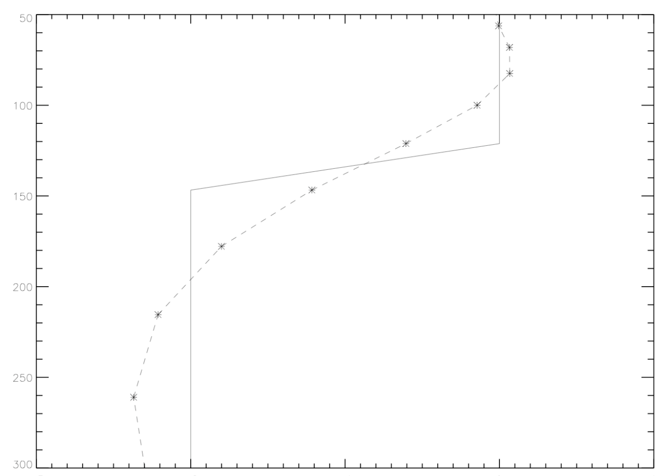
MLS temperature has cold bias with IWC. It means that a bigger IWC would lead to a bigger cold bias. That's why I try to compare zonal mean temperature anomalies (Figure 4b) with zonal mean IWC with relative distance from cloud top height.





Left figure shows the same condition in this study (Used all valid MLS data). Zonal averaged IWC (dash line) is up to 8 mg/m^3, but their peak is about 1 km below the maximum cooling anomalies. For the thin clouds, IWC is less than 1 mg/m^3. To understand IWC effect on temperature, we used MLS profiles with IWC < 1 mg/m^3 above 200 hPa (Right figure). About 78 % of MLS profiles satisfy this condition. The maximum cooling anomalies in thick clouds reduce less than 0.5 K, and there is still up to 1.5 K cooling below cloud top height. This figure shows that it seems like the cooling anomalies are robust, and atmospheric.

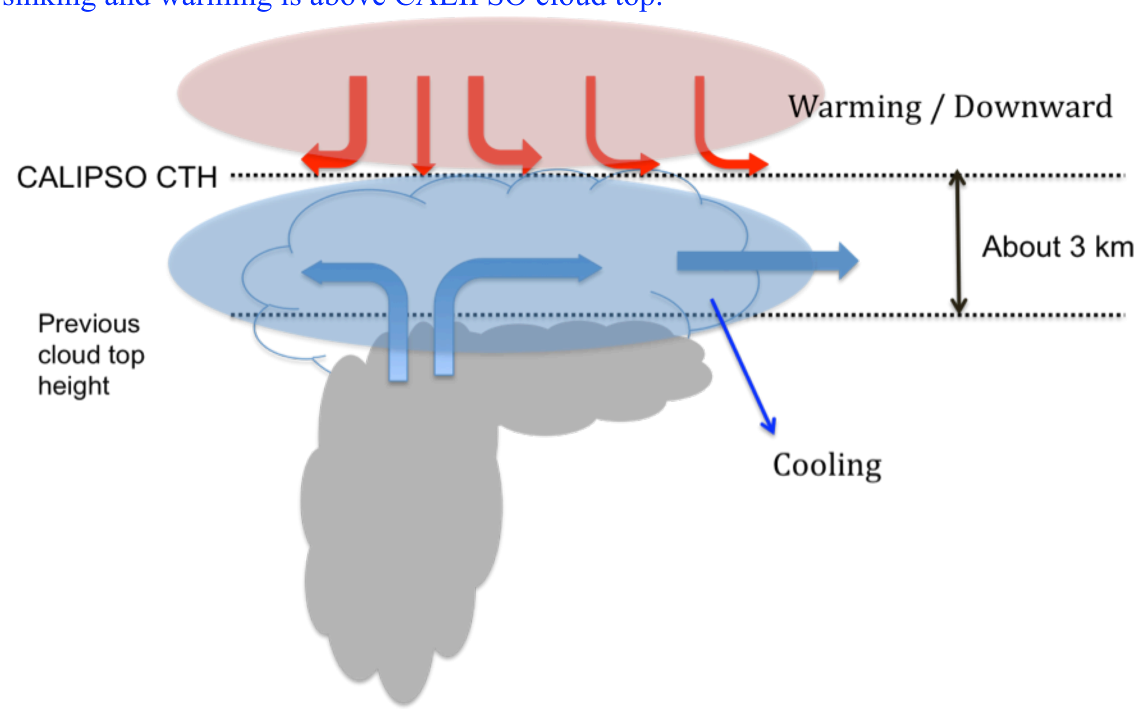
Additionally, MLS temperature might be smoothed due to averaging Kernel effect. Following figure is one extreme example of an averaging kernel effect (performed by Bill Read).



This figure shows that the warm anomalies above cloud top are a robust feature and not merely smoothing an artifact of what is happening down low. The figure shows two curves. The thin line is representative of truth and the dotted with * is how MLS would smooth it. Lets assume for sake of discussion that you have a cooling anomaly in the cloud and extends downward. The cooling anomaly at 178 and 147 is a real effect from overshooting convection and the cooling anomaly at 215 and below could be in cloud retrieval artifacts. Lets also assume no anomaly above the cloud. This is represented by the thin line. The dashed and * line is what MLS would retrieve as a result of the AK smoothing. As you can see except right at the cloud top interface (between 150 and 120 hPa) MLS is adding a few tenths of a K to the "true" situation. Therefore since the warming anomalies are over 1 K, I think this is robust and real, even if there are some cloud induced artifacts happening at 250 hPa or so.

4. 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.

I explained about it in conclusion section (line 27 on page 8976 to line 9 on page 8977: page 18 in word file format). Because CALIPSO cloud top height is about 3 km higher than that in previous work like MODIS and AIRS (e.g., Weisz et al., 2007), Level considered above clouds in previous work is below cloud top height in our analysis. A schematic is shown in following figure. In previous cloud information, cooling occurs near and above cloud top, but this paper shows that cooling is below CALIPSO cloud top because CALIPSO cloud top height is 3 km higher than that of previous studies. The air sinking and warming is above CALIPSO cloud top.



5. The authors imply that any data with greater than 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.

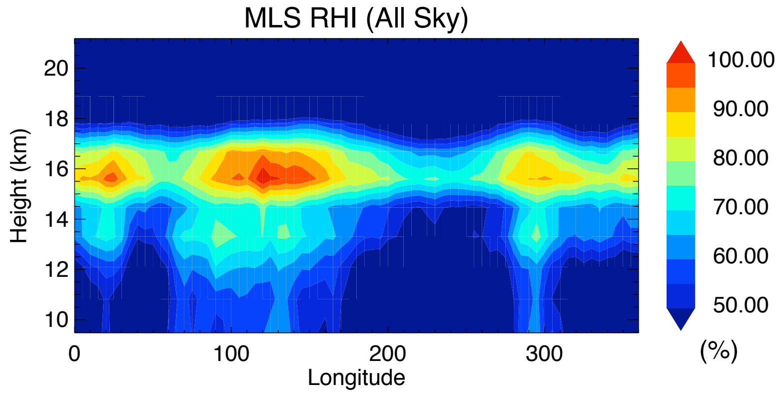
First, I want to note that I do not use original MLS relative humidity data, which might have huge error due to poor temperature precision. As mentioned in "data" section, we used new temperature data which is modified after comparison to GPS temperature data (to reduce MLS temperature bias), and then re-calculated relative humidity with respect to ice. Figure 6 seems to show reasonable behavior among q, T, and RH overall in the range of ± 0.8 K MLS temperature precision. Many previous aircraft and satellite studies also shows that supersaturation with respect to ice occurs frequently even clear sky in upper troposphere (Krämer et al, 2009; Jensen et al., 2005).

6. 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 agree that in situ cirrus cloud formation make water vapor reduce. I add some explanation about in-situ cirrus generated by Kelvin wave activity. Actually, in-situ cirrus generated by Kelvin wave shows the same result which is cooling temperature and reducing water vapor though cooling temperature in this case is not the result of cloud mixing but pre-existing condition for forming cirrus.

7. 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.

I want to note that relative humidity used in this paper is always with respect to ice. This statement (line 23 on page 8973) does not mean that air above 16 km is always supersaturated. This statement explains Figure 8 (now figure 7), which shows relative humidity below cloud top in cloudy condition. Therefore supersaturated condition occurs below cloud top when cloud penetrates the level we observe. Many in situ measurements from aircraft also show frequent supersaturation with respect to ice near and inside clouds (e.g., Krämer et al., 2009). The relative humidity with respect to ice in all sky condition is shown at following figure, and only small area (near 16 km over warm pool) has 100 % relative humidity.



8. 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.

The reviewer is confusing between ozone and water vapor. I described that air sinking cause increasing ozone mixing ratio (positive anomalies) because ozone increase with height but decreasing water vapor mixing ratio (negative anomalies) because water vapor decrease up to 18 km (above the cold point).

9. 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.

We tried to answer about data issues used in this paper, and believe that MLS temperature and water vapor data is reliable and their anomalies above and below CALIPSO cloud top is robust. I modified Fig 10 (11 in previous version) by adding other possible case described in this paper.

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.

I agree this comment because several previous studies have already shown the similar argument. Therefore, I omitted detail description.

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.

Dot line indicates zero value.

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

I changed it. Thank you.

Reference

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