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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the deep convective temperature signal within the tropical tropopause layer (TTL)" *by* L. C. Paulik and T. Birner

Anonymous Referee #1

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This paper shows some interesting diagnostics (e.g. Figure 3), and I think makes a good case that only the highest convection influences TTL temperatures. I recommend publication provided the comments below are addressed. The comments that would require new calculations are 1 and 4, and possibly 2.

Major Comments

1. Ozone mixing ratio height

I think care should be exercised when using this diagnostic during seasons when rainfall is low. During dry seasons, the seasonally averaged ozone mixing ratio at the LNB will be larger. For an individual ozone profile, in which the ozone mixing ratio height is

defined as the highest altitude at which ozone is lower than the seasonal ozone mixing ratio at the LNB, one would therefore place the ozone mixing ratio height at a higher altitude. The ozone mixing ratio height will therefore start to lose meaning when the upper tropospheric ozone mixing ratios become decoupled from boundary layer mixing ratios, due to an absence of convection. I would have been more comfortable with an ozone mixing ratio that was related to the seasonal mean value of ozone in the boundary layer, and would recommend sensitivity studies using this alternate definition. I suspect you would get a stronger seasonal variation in the blue curves of Figure 9.

2. Absence of Lower Tropospheric and Surface Cooling from Deep Convection.

It is well established from observations and CRM's that deep convection is associated with cooling in the lower troposphere and boundary layer, especially in the near field (Mitovski, J Clim 23, July 1, 2010; Mapes J Clim 22, Jan 15 2009; Mapes and Houze, JAS, May 15 1995; Mapes, Dyn Atmos Oceans 42, 2006; Benedict and Randall, JAS 66 Nov 2009; Mitovski JGR 117, 2012). Although the focus of this paper is on the upper troposphere, Figure 7 shows only a tiny hint of a cooling near the surface. This inconsistency with previous analyses should be discussed. I suspect the main reason for the difference is that GPS COSMIC temperature is not good enough below 5 km to show the lower tropospheric and boundary layer cooling. But there may be methodological differences also (e.g. using clouds rather than rainfall as a proxy for convection).

3. "Convective Influence"

The paper sometimes uses the term "convective influence", as in, "the conventional LNB appears to effectively define the maximum vertical extent of convective influence." (page 19634). However, there are many ways to define convective influence. In the case of chemical tracers, one mainly cares about the convective detrainment profile. In the case of clouds, one mainly cares about the highest altitudes to which clouds can rise, and the locations in which the temperature perturbations associated with convection can give rise to supersaturation (often disconnected from the mass outflow itself).

A well known problem here is that cloud top height is often very different whether one is using a thermal or visible signal, so there is an intrinsic ambiguity in its definition. In the case of temperature perturbations, one cares about the locations to which convectively generated waves can propagate, and also the radiative decay timescale (since the height variation of this timescale has such a strong influence on the amplitude and persistence of the temperature perturbations.) In the case of of lower stratospheric ascent forced by dissipating Rossby waves, one could say convective influence extends well into the stratosphere. There is no single way to define a specific highest height of convective influence, and I think it is confusing to the field to think that there is a unique definition. In the case referred to here, it seems as if the author is implicitly defining convective influence as the maximum height to which convective clouds extend. In this case, it is much better to simply be specific about the particular physical process you are referring to, and avoid the term “convective influence” as overly vague.

4. Larger distance of temperature anomalies during DJF

This comment refers to Figures 7 and 8. From what I understand, these figures only use the latitude range where the sun is directly overhead, so (20S - Eq) for DJF and (Eq - 20 N) for JJA. The fraction of convection over land in the NH during JJA is likely significantly higher than in the SH during DJF. So one possibility is that the differences are due to land/ocean differences rather than intrinsic. For example, if convection over land more easily reaches 17 km for a given rain rate (this seems plausible), then the differences in the temperature anomaly patterns could arise from differences in the rain rates, i.e. the JJA temperature anomalies are weaker because the rain rates corresponding to clouds above 17 km are on average weaker. One could therefore repeat the analysis using a TRMM rainfall threshold rather than a cloud occurrence above 17 km threshold.

Minor Comment:

line 15, page 19633: "In contrast, the LRM and ozone minimum height do not appear

to represent deep convective cloud tops well." Yes this would be expected, since the ozone minimum feature is caused by a maximum in convective detrainment, which is several km below cloud top.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19617, 2012.