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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

G. Kiladis (Referee)

george.kiladis@noaa.gov

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This paper convincingly demonstrates a strong temperature signal associated with the deepest tropical convective events as determined by ozone deficit signals along with satellite estimates of very high cloud tops within the tropical tropopause layer (TTL). Although the results are a bit noisy due to sampling issues, and may in fact be a bit contaminated by lower frequency signals (see below), the approach paves the way for further research using these diagnostics. For example, the technique could be applied immediately to examine the impacts of equatorial waves on the TTL, and the sampling within the CloudSat and GPS temperature data sets will only improve over time. I have

only relatively minor comments on this first draft of the paper, and most of these have to do with aspects of the presentation. In my opinion the paper raises many interesting questions, and in the end concludes with the important result that it is only the deepest convective clouds that have a significant impact on the TTL. My detailed comments are given by page number:

Pg. 19620, Line 6: "diverges" suggest "deviates".

Line 8: "predominantly subsides" suggest "becomes less dominant"

Line 11: "Pacific"

Line 15: "upper tropospheric warming" since negative lower tropospheric temperature anomalies are generally associated with deep convection, despite Fig. 3 (see below).

A minor suggestion: In my opinion it is preferable to say "tropospheric warmth" here and in other places rather than "tropospheric warming" (or cooling) since the latter implies a temperature tendency, whereas the temperature signal itself is a result of the tendency.

Pg. 19623, line 17: Fig. 2 is quite interesting. It would be useful to have some information on how often CloudSat covers a given location (the sampling frequency). Also, in Fig. 2, does each pixel shown represent just one event? Can there be more than one event at a given point represented on the map? I suppose another way of asking this is: what is the total possible number of events in Fig. 2 compared to what is shown at each point?

Lines 22 and 24: You may want to say "between 20S and the equator" The reason for restricting to this latitude range isn't given until later, although there are still plenty of events well north of the eq. shown in Fig. 2.

Pg. 19624, line 13: I am not sure it's necessary to show eq. (1), the discussion on the impact of water vapor seems sufficient without it.

Line 20: “without the water vapor” I suggest saying something like: “taking into account the water vapor contribution to the refractive index”

Pg. 19625, line 4: I guess you mean “similar timescales of variability”?

Pg. 19626, line 10: “A warm convective temperature signal”

Pg. 19626: What are the approximate sample sizes used to construct Fig. 3 (and later figures)?

Line 9: “difference in means” you mean a t-test?

line 15: “strongest convective signal” refers to the upper tropospheric warm anomaly.

It is interesting that in Fig. 3 you have uniformly positive temperature anomalies throughout the troposphere, since the lower troposphere is typically cold during deep convective events identified by other means (e.g. Johnson and Kreite; Sherwood and Warhlich; Kiladis et al. 2009). Not completely sure how to interpret this, but there may be a low frequency signal present since the method used to calculate the temperature anomalies (by just subtracting a daily average) would leave interannual variability intact. For instance, ENSO has a strong influence on convection and tropospheric temperature at Samoa, and as I'm sure the authors are aware, the ozone signal also (e.g. Oman et al., 2011 GRL pg. L13706). Have the authors considered these impacts on their signals? One way to get around this might be to high pass filter the temperature data with, say, a 120 day cutoff, removing lower frequency variability. While this may not be worth doing for the present paper, these potential issues should still be mentioned.

Pg. 19627, line 2: “it is the mean level in the “S” shaped. . .”

The determination of the ozone mixing height needs to be better described. On line 24, does “each season” mean that the average ozone at the LNB is determined for the standard seasons (DJF, etc) and these are used as the climatology? If so, then on pg. 19628 I assume that 33.4 is the average value for March-May at the climatological LNB for that time of year, is that correct? Further clarification is needed.

What do the results from other stations look like? Similar enough to just say so in the manuscript?

Pg. 19630: Another interesting signal in Fig. 6a for moderately high cloud tops is the warm anomaly centered at around 20 km in the lower stratosphere. This is also a typical signal associated with propagating waves coupled to convection (Kiladis et al. 2009) and the MJO (e.g. Kiladis et al. 2005 JAS pg. 2790). It seems like that signal is also present for 16-17 km cloud heights, but is not statistically significant. Not sure why that would be the case, do the authors have any ideas about this?

Pg. 19631: “anomalies in DJF are more persistent” not sure that's the best choice of words, maybe “widespread” instead?

It initially sounds here like there is an assumption that an individual cloud event leads to the signal, but isn't it likely that multiple clouds with tops at or near 17km could occur over a wide region, especially if they are organized by wave activity? Evidence presented next in Fig. 8 supports that, but I suppose that it is not possible to deduce this for multiple cloud events using CloudSat due to the sampling. The possibility could be mentioned as a lead in to the following paragraph.

Fig. 8 supports the idea just mentioned above: it is likely that deep cloud clusters are responsible for the signal shown here, since it is unlikely that there would be such a strong temperature signal for so many days, especially prior to one lone deep convective event. Based on the timescale involved (more than a week), my guess is that you are preferentially sampling MJO events. This might be worth a follow up study. It wouldn't be difficult to isolate MJO activity based on filtered OLR, for instance, associated with the samples in Figs. 7 and 8. Have the authors thought about this possibility?

Fig. 9 caption needs work. Shown is the “relative frequency” (contours), I assume this is the number of clouds at a given height over the total number of samples within 1000 km, but that should be stated. It these are really based on monthly data then the extra tick marks between the month labels are not needed.

The result that “the LNB and ozone mixing height tend to occur at roughly the same altitude as the convective cloud tops” is sketchy. The most that can be said is that these quantities have roughly the same seasonal cycle in terms of their height, and if anything the LRM and ozone minimum height have a better correlation to each other and to the lower boundary of the highest cloud top signal. Interpretation is difficult, and my impression is that the sampling is not enough to reduce the noise here.

In Fig. 9b the eye is drawn to the local minimum of ozone with less than 25 ppbv occurring at 10-13 km during the convective season, and the is a local maximum of greater than 40 ppbv at 6 km during the non-convective season. So it might be better to say on line 20 something like: “we can trace the 30 ppbv contour from ∼14 km. . .” rather than how it is stated in the text. Actually the ozone signal is lagging the convective signal, with the upper trop minimum occurring in Feb.-March and the lower trop maximum in Sept.-Oct. Thompson et al. (2011) don’t attempt to explain the former lag although it appears to be present at other maritime stations as well, and it seems that the Sept.-Oct. signal is dominated by the SH fire season even at Samoa, according to them. Not sure if you want to get into any of this here, in fact in light of the uncertainties in explaining the details of Fig. 9, it may be best to abandon showing that figure at all. As it stands the main conclusion on pg. 19634, that “it is only the highest deep convective events that have a significant impact on the TTL”, appears to be solid and not dependent on Fig. 9 at all. I will be curious if others agree with this in the discussion of this paper in ACPD.

Signed,

George Kiladis

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