Response to Referee #2 (George Kiladis)

We thank reviewer George Kiladis for his thoughtful and thorough review, which should lead to a number of improvements in the presentation of our material. Below are point-by-point responses, with the reviewer comments in normal font and our responses in italic font.

Pg. 19620, Line 6: “diverges” suggest “deviates”.

done

Line 8: “predominantly subsides” suggest “becomes less dominant”

done

Line 11: “Pacific”

done

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

done

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.

In principle agreed on the precise meaning of “warming” vs. “warm(th)”. However, the signals arise as a response to mid- to upper tropospheric (latent) heating, which does describe a temperature tendency. In that sense “warming” and “cooling” may be justified. We have tried to clarify the text where necessary (i.e. we now use “anomalously warm/cold” when specifically referring to temperature, but leave warming/cooling in a few places where referring to convectively induced tendencies).

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).

Now provided (CloudSat crosses the equator about 14 times a day – it crosses the same location every 11 days, i.e. ~9 times per DJF season or 45 times during all DJF seasons analyzed). CloudSat overpasses are at 1:30 am and pm, which likely produces some biases with convection over land which tends to peak in late afternoon.

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?

In principle there can be several events per pixel. This happens much more so for the 15 km threshold. For the 17 km threshold pretty much all pixels represent just one event. Our main goal here is to provide background information on the geographical distribution of deep convective cloud tops above some threshold; the plot does not quantify frequency of occurrence or similar, which would require some sort of gridding.

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.

done

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.

We feel the equation provides the most straightforward way for the reader to judge the relative contributions due to temperature and water vapor to the refractive index and have therefore kept it in the paper.

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

we have modified the text to "... temperature profile that is consistent with both that water vapor profile and the refractive index ..."

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

Modified to "correlated variability between upper tropospheric ozone and the lapse rate".

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

we refer to both the warm upper tropospheric and the cold signal aloft, text modified.

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

The data set contains 425 profiles in total (which is the sum of the composite groups in Fig. 5) – 10% therefore amounts to 42 profiles. Information added to figure caption. Note later figures already contain information on sample sizes.

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

Yes, text modified.

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.

Reviewer 1 raises a similar concern – please see response to her/his comment 2. We have done some sensitivity testing concerning the ENSO influence. Reduced ozone events do indeed preferentially occur during the warm ENSO phase. Our temperature response also looks similar to the ENSO response (e.g. Randel & Thompson, JGR 2011). However, when reduced ozone events are studied separated by ENSO phase the typical temperature signal as in Fig. 3 results for both the warm and the cold ENSO phase (i.e. looking at the 10% lowest ozone events during La Nina conditions vs. the 10% lowest ozone events during El Nino conditions), that is relative to the average temperature for each phase. Other low frequency signals such as the MJO likely also have an impact and relevant discussion has been added.

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

done

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.

The threshold refers to the average of O_3@LNB, which is different from ozone at the average LNB. Yes, seasons refer to standard seasons. Text has been modified to clarify details of the definition. Furthermore, a table is now provided, containing the threshold values for each season.

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

Fiji gives very similar results to Samoa, which is not surprising given it's similar location and *pristine marine environment. Other stations, that are either not located in frequently convecting regions, or have more polluted boundary layers, do not show as clear signals as Samoa and Fiji. Corresponding discussion added to text.*

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?

We think that our analysis averages over several different wave types, with different structures and frequencies. That is, only the common features of these different wave types survive in our analysis, and these are the deep tropospheric warm and TTL level cold anomalies. Other features, such as repeated warm and cold anomalies in the lower stratosphere are likely averaged out. The MJO likely projects onto our analyzed structures and it is indeed interesting to find some similarities between previous studies (your 2005 paper, and your earlier 2001 paper in QJRMS) and ours. Statistical significance for the warm anomaly around 20 km likely drops for cloud tops above 16 km because the sample size drops (see sample numbers given in Fig.). It is also important to point out that some of the warm anomalies near 20 km come from anomalously <u>low</u> cloud tops and seem to represent an "opposing" signal to the cold CPT anomalies for very high cloud tops. Related discussion added to text.

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

Done.

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.

We did not intent such an assumption to come across, in fact very high cloud tops are most likely to be found in large-scale organized cloud complexes. The observed temperature anomalies should be interpreted to arise from the cloud complex as a whole, consistent with the large-scale and long-lived nature of these anomalies (as suggested by your comment below).

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?

Agreed. We have extended the analysis shown in Fig. 8 to include much larger lags and a low-frequency signal reminiscent of the MJO seems to appear. However, structures are quite noisy given the relatively small sampling presently available. Relevant remarks added to text, and we still only show lags up to +/-14 days in Fig. 8. We agree that this is worth a follow-up study and we thank the reviewer for his encouragement in this regard.

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.

Thanks – caption modified.

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.

Text modified to: "The LNB and ozone mixing height tend to fall within 1 km of the height of the maximum frequency of deep convective cloud tops, with similar seasonal cycles. In contrast, the LRM and ozone minimum appear to more closely represent a lower bound of deep convective cloud tops, with a typical separation of 2-3 km between either the LNB or ozone mixing height and the LRM or ozone minimum height".

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.

It is true that there appears to be a ~1 month lag between the ozone signal and convective clouds. However, the upper tropospheric ozone minimum during Feb-Mar is in phase with the lowest boundary layer values. Furthermore, mid-tropospheric ozone, which enters the updrafts through entrainment is higher during Dec-Jan than during Feb-Mar. That is, the maximum convective impact on upper tropospheric ozone likely occurs in phase with the most frequent clouds. Our main point here is that, by comparing the cloud frequency distribution in Fig. 9A with the ozone distribution in Fig. 9B, the convective signature in ozone can be traced fairly well during the convective season. In that sense we find Fig. 9B useful. We have modified the text to more clearly make this point.