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Interactive comment on "Observations of convective cooling in the tropical tropopause layer in AIRS data" *by* H. Kim and A. E. Dessler

Anonymous Referee #2

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Review of 'Observations of convective cooling in the tropical tropopause layer in AIRS data' by H. Kim and A. E. Dessler

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Foremost, I would like to mention that I strongly believe that scientific reviews of a given paper should be independent. Therefore, I did not take into account in any sense the already published review (30 Nov). However, I will take it into account once I submitted my own review. (This should not constitute a problem for the publishers since it is possible to submit several comments to a paper). Anyway, I apologise for any potential duplication in what follows compared to the existing review.

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There is an ongoing debate in the literature concerning the relative importance of convection vs large-scale slow ascent in determining the thermal structure as well as the degree of dehydration in the tropical tropopause layer (TTL). The authors try to contribute to this debate by analysing two months of temperature profiles measured by AIRS in conjunction with NCEP/AWS images of brightness temperature. By employing a method developed previously by Sherwood and Wahrlich (1999) (hereafter SW99) they devide the temperature profiles into different convective 'stages'. They find progressively stronger negative temperature anomalies at the cold point (CP) as convection 'developes'. A cooling rate between 7 and 9\,K/day is estimated for the active stage of convection.

The new dataset certainly bears with it the potential for important new results. However, at the present stage of the paper I am not convinced by the utility of the methods and thus by most of the conclusions the authors draw (details below). Publication in my view requires a much more careful and detailed analysis. Therefore, I can only recommend rejection of the paper in the present form but strongly encourage resubmission once the major issues have been taken into account and the method has been further validated for the present purpose.

==================== Reasons for rejection ====================

1)

As far as I know the dataset has not been used previously, at least not to study the TTL. However, there appears no attempt in the paper to validate the data. The authors don't even show a mean temperature profile so that one could try to compare that with other observations or meteorological analyses. One has to assume that the authors did do such a comparison, indeed - what did it yield? If you try to explore what drives the temperature structure of the TTL you should at least show that a TTL is present in your dataset and discuss how it compares to previous observations.

Furthermore, it should be justified in some way that the given vertical resolution of 1 km is sufficient to resolve and study the convective influence on the TTL's temperature. One would expect, e.g., that convection alters the pressure of the CP which to a large extend remains unresolved in the AIRS data.

2)

All the major results of the paper crucially depend on the validity of the method developed by Sherwood and coworkers (SW99 and Sherwood et al. 2003). While the latter apply the method to the Pacific warm pool only (where there is ample convection) and perform a number of tests to show the validity of their method, it remains an open question whether the method can be applied to other areas as well. One of the potential difficulties mentioned in SW99 is the upper-tropospheric wind shear that might substantially offset the high clouds from their convective origin (horizontally). A horizontal wind speed of 10 m/s, e.g., transports air from one edge of the 1x1 box to the other within 3-4 h, i.e. within about a quarter of the temporal resolution of the AIRS data. It is therefore quite likely for a given temperature profile that is classified as convective to be only indirectly influenced by convection. This issue certainly requires a much more detailed analysis than in the present stage of the paper. One possibility would be to distinguish systems that build up in a certain box from systems that move into it.

Furthermore, there arises a problem in the physical argumentation as follows. The CP-air in a given box might indeed be diabatically cooled by convective overshoots but subsequently transported adiabatically into a neighbouring box. Since this air will have a smaller potential temperature (theta) than the CP-theta of the neighbouring box it will end up correspondingly lower within the TTL. Specifically it will not lead to cooling at the CP in this neighbouring box. The actual and overall convective effect on the CP becomes very questionable in this scenario, especially when faced with the frequency of 'convection' measured in the the current analysis (3%, page 7623, line 19). One should note that the actual frequency of convection that reaches the CP might be still much smaller than these 3%. In fact, the temperature anomaly at the CP in stage 3 is only ˜2K (there is no unit given but I assume that it is K). One can therefore estimate the temperature anomaly in non-convective areas (the remaining 97%) to be ˜0.05K, i.e. almost indistinguishable from 0. That is, the given fraction of 3% (in time and space) implies very small influence by convection on the mean temperature at the CP.

3)

The estimation of the cooling rates (Fig. 2) appears not to be physically meaningful to me for the following reasons. First, one cannot associate a physical process with what is plotted in Fig. 2 since it merely shows different temperature anomalies (that represent different places and times, i.e. 'different convection') as a function of time since this individual 'convection' started. Second, it is not at all clear to me why there should be linear behaviour in such a plot (in fact, the behaviour is not really linear, especially in July where the linear fit does not seem to be justifiable). Third, one expects the variability to become larger the larger the time since 'convective' onset which would have to be taken into account in any attempt to fit the data (linear or nonlinear). What would you get, e.g., if you scatter-plotted every anomaly with its respective time since 'convective' onset and fittet this data? How much variability is associated with such an estimated 'cooling rate'?

I think that the only way to estimate cooling rates out of the data is to average individual cooling rates (the latter estimated as individual temperature anomaly minus mean temperature anomaly at the time of 'convective onset'(!) devided by the time since 'convective' onset).

A note on the plotted symbols in Fig. 2: the whole stage 2 appears to be offset by +1 h (time since 'convection' started cannot be $>$ 3 h according to the definition of stage 2) which makes the linear fit to appear better than it actually is. Am I missing something? Further, some bins do not have a symbol (e.g. at 4.5, 10.5, 13.5, 17.5 h for Feb) - why?

—————————- Some proposed analysis —————————-

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- my suggestion is to plot temperature anomalies (without assigning them a stage) versus 'convective fraction' (incl. testing different thresholds in brightness temperature) - does that yield any significant relationship? you could then start to devide temperature anomalies into 'convective' stages.

- illustrate the method for one specific convective event. one could furthermore show the evolution of brightness temperature averaged over one 1x1 box as well as the evolution of fractional cloud coverage (defined by several thresholds) for one event. This should help the reader to get a better feeling about the method used.

——————————- Further, general minor comments ——————————-

Comments to Fig 1. Frequency distributions of the temperature anomalies are most likely strongly skewed (especially for stage 3). One just has to consider the fact that most of the anomalies are negative. If this is true it would be interesting to see median anomalies as well. Furthermore, it is not possible to get a sense for the number of profiles involved in each stage - please provide a measure (e.g. percentages). The fact that there is no diurnal cycle should be discussed in more detail - wouldn't you expect to see a diurnal cycle given the results by Soden (2000, GRL 27, pp 2173) and Tian et al. (2004, JGR 109, doi:10.1029/2003JD004117)?

Comments to Fig 3. The dependence of both, mean anomaly and cooling rate on the threshold in brightness temperature does not seem to be consistent when varying the fractional threshold - why? Is this an indication that the method has problems in here? In fact, the authors argue that the plot justifies their method, however, the large spread in cooling rates even for fractional thresholds within the range 10-30% rather suggests the contrary.

Convection is a mesoscale phenomenon. Characteristics are e.g. strong heating within the bulk of its updrafts and weak cooling in the surroundings of these updrafts. Local updrafts often comprise not much more than a few 100 km^2. The precise location of the temperature profile relative to the convective updraft is obviously very important

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concerning the effect of the convection on the thermal structure.

——————————- Specific minor comments ——————————-

Page 7616

- abstract, line 9: "variations by season" is certainly too strong, one month of each season and only one is certainly not enough

- line 18: the term tropopause is ambiguous in the tropics, one should avoid this term completely and decide on a physical meaningful term instead, e.g. cold point; at this point within the text the term TTL is certainly most appropriate

Page 7617

- line 25: phrase "cold air that detrains" (also at other places): this is misleading, it is either warm air that entrains into cold air or just turbulent mixing of warm and cold air

Page 7618

- line 12: why is the data limited to over ocean?

- last para: point out more clearly that method used closely follows SW99; brightness temperature as a measure for convective cloud tops has its uncertainties, please comment on this

Page 7619

- definition of stages is confusing: they do not necessarily need to follow each other - do you have an extra criterion on this? e.g. line 22: you have to additionally impose this condition otherwise this statement is wrong as far as I can see

Page 7620

- line 28: I cannot find this 10 K in Kuang and Bretherton (in their Fig. 4, cooling events lead to a mere 0.2 K), the result in their Fig. 6 is hypothetical and just meant to qualitatively indicate the convective influence

Page 7621

- first para: if this data is invalidated you cannot comment on it

- line 11: please state that this cooling rate is concerning Theta (also caption of Fig 2)

- line 15: Kuang and Brethertons few tenths of a K concern the overall mean, not just convective events!

- line 21: the dependence is on the threshold of fractional coverage

Page 7622

- line 2: I cannot support calling the range 5 to 10 K/day an insignificant variation

- lines 12-17: SHZ03 really show a cooling (in temperature) whereas your description solely based on potential temperature does not necessarily show cooling

- next para: it is not clear to me if your diurnal cycle (viz. its nonexistence) is meaningful - so it is not clear if you can use it for this discussion

Page 7623

- the description of calculating the entrainment rate is misleading, a simple mixture of environmental and ascending air would give d(theta) = dr*(theta_a - theta). however, if you take into account that the mixing is an ongoing process the environmental theta will change during the mixing which gives eq. 1

- I cannot see the reason for using the cooling rate in eq. 2 - in fact, you already have an estimate for d(theta) in Fig 1, stage 3 (being about 2 K), btw you get the same d(theta) from Fig 2 by taking the cooling rate (˜6 K/day) and multiplying it with ˜8 h (estimated timescale out of Fig 2)

- using this 2 K for d(theta) your entrainment fraction is in the order of 10%

- line 15: the 44% are meaningless, either your timescale for convection is 10 h, or 1 day

- I cannot see the sense in calculating that mean entrainment rate with the aim of explaining the cooling rate out of Fig 2 - the latter is only representative for the convective events (3% of all profiles), so what does this mean entrainment rate mean physically?

Page 7624

- line 15: "might be very dry" - but does not necessarily have to be the case, convection could bring large amounts of moisture up to the CP at the same time and thus not lead to dehydration, see Kuepper et al. (2004, JGR doi:10.1029/2004JD004541)

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7615, 2004.

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