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Atmospheric Chemistry and Physics Discussions

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

H. Kim and A. E. Dessler

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We are thankful to the reviewer for the detailed comments and recommendations. Here is a list of major modifications, and answers to specific comments are listed below.

1. We have added an example of a convective event to show how each convective stage is determined (Fig. 1).
2. Figures 3 and 4 were also modified to include error bars.
3. We have clarified arguments on the sensitivity test.
4. We have modified equations for the calculation of turnover time.

Reviewer: 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.

Answer:
- We have added validation literature for the AIRS data set. Observations from satellite sensors on board the Aqua satellite of temperature, ozone, water vapor and cloud properties in the upper troposphere and lower stratosphere show good agreement with aircraft observations from a recent campaign. Temperature is generally within +/-1.5 K of aircraft observations [Gettelman et al., 2004].
- Our study mainly focuses on the temperature 'anomaly' at the fixed pressure levels, rather than the absolute value of the cold point temperature. The vertical depths of cold/warm temperature anomalies are several kilometers (e.g. Fig. 2), so we believe that lack of vertical resolution has little effect on our result.

Gettelman A., E. M. Weinstock, E. J. Fetzer, F. W. Irion, A. Eldering, E. C. Richard, K. H. Rosenlof, T. L. Thompson, J. V. Pittman, C. R. Webster and R. L. Herman Validation of Aqua satellite data in the upper troposphere and lower stratosphere with in situ aircraft instruments, Geophys.Res.Lett, 31, L22107, doi:10.1029/2004GL20730

Reviewer: 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

Answer:

- We agree that the horizontal transport is not a factor that can be ignored without a test. However, Sherwood et al. (2003) have validated this method by testing both cases with/without horizontal winds. We also performed a similar test; the results did not show any significant difference in either the cooling amount or the cooling rate estimation. We wonder if the reviewer has any specific reason that this method should fail out of western Pacific region.

- Also, there seems to be a misunderstanding in the reviewer's argument about the temporal resolution of our data. It should be noted that NCEP/AWS image data has half-hourly temporal resolution. For each AIRS temperature profile, the difference between the time of AIRS measurement and the time of NCEP/AWS brightness temperature measurement is less than 15 minutes. It is enough to consider that both measurements were performed almost coincidently. We investigated within 3 hours before and after the time of the AIRS measurement. During this time period, the location of the temperature measurement is still in the box or near the box.

- Though the reviewer says that the horizontal transport could remove all the convective cooling effect, the change of heat budget by horizontal transport is not enough to cancel out all the thermal impact by the vertical transport (e.g. Sherwood (2000) Fig. 3).

- Please, note that this study is not about the global heat budget. We focus on the convective region, and it is hard to agree that 2K cooling in the convective region is just meaningless. We do not argue that deep convection is the only mechanism to cool the whole tropical tropopause. Localized cooling over convective regions still has an important potential to affect this region's water vapor regulation. For example, Holton and Gettelman (2001) pointed out that air parcels can travel horizontally several thousand kilometers at altitudes near the cold point while ascending just a few hundred meters, so a particularly cold region localized in longitude might dehydrate a large fraction of the ascending air.

Sherwood, S.C. A "stratospheric drain" over the maritime continent. Geophysical Research Letters, Vol. 27, No. 5, 2000, pp. 677-680.

Reviewer: 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).

Answer:
We have tried to calculate the average of cooling rates of individual points, as the reviewer suggested, and had a cooling rate of -10 K/day. We agree this is the best way to calculate the cooling rate in a theoretical point of view, but this method doesn't represent well the status of active convection in the longer duration because lots of points are congested near the convective onset. This result, -10 K/day, seems to be consistent with the cooling rate just near the convective onset (see Fig.3). To avoid this problem, we first calculated averages of temperature anomalies in 1 hour-bins, and fitted a line to them. This is shown in Fig. 3, and this method shows the evolution of convective cooling as times goes on after the convective onset better than other methods. The reviewer says "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". However, it should be noted that the temperature anomaly used in this method is a deviation from the 'local' mean temperature that represents non-convective status of specific time and location. Therefore, this temperature anomaly shows how much temperature changes as a convection progresses. We cannot agree that averaging of these temperature anomalies is just meaningless.

Reviewer: 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?

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Answer: We have corrected this mistake. Some symbols are missing because there are few points in those bins.

Reviewer: - 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.

Answer: We have added a figure (Fig. 1) to show an example of convective stage determination. This plot also shows how the convective stage determination is affected by the change of Tb and fractional thresholds.

Reviewer: 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).

Answer: Please, note that stage 0 is not included in this plot. Negative anomalies in stage 1-5 are balanced by the positive values of stage 0.

Reviewer: 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)?

Answer: We disagree. We do not argue that temperature has no diurnal cycle, rather that we could not find any difference in the diurnal separation of temperature 'anomaly'. If the observed cooling in our result is due to the cloud-top radiative cooling, one would

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expect a significant difference between daytime and nighttime results because solar radiation makes an important contribution. We could not find any difference, so we concluded that this observed cooling cannot be explained by the effect of cloud-top radiative cooling.

Reviewer: 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

Answer:
- It can be easily explained by Fig. 3 (The cooling rate plot). Theoretically, a higher fractional threshold means strong convection, and strong convection is generally associated with longer duration. That is why there exists a dependency between the mean anomaly and the fractional threshold. On the other hand, the cooling rate is the 'slope' of this anomaly-duration plot, so it is not affected by the change of duration, or the change of fractional threshold.

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

Answer: We have revised the sentence.

Reviewer: - 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

Answer: We have revised the sentence.

Reviewer: - 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

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Answer:
We disagree. Deep convection that reaches near the 100-hPa level certainly overshoots it level of neutral buoyancy. If air detrains from this deep convection, it should be much colder than the environmental air.

Reviewer: Page 7618 - line 12: why is the data limited to over ocean?

Answer: As mentioned in section 2 of the paper, data retrieval is done over ocean only for current version.

Reviewer: - 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

Answer: We have added a sentence about this.

Reviewer: 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

Answer:
We don't have any extra criterion. Convective stages are not following each other because the duration of stage 3 can vary.

Reviewer: 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 - 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!

Answer: We have corrected these.

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

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Answer: We agree. We have mentioned that in the paper.

Reviewer: Page 7622 - line 2: I cannot support calling the range 5 to 10 K/day an insignificant variation

Answer: The most important point in our result is that this thermal impact is 'cooling' and its magnitude is not explainable by other reasons such as cloud-top radiative cooling. The variation between 5 - 10 K/day doesn't change our conclusion.

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

Answer: We have revised the sentence.

Reviewer: - 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

Answer: This is answered above.

Reviewer: 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 44day

Answer: We agree. We have revised most of the equations and the arguments on the turnover time estimation, and removed the timescale of convection.

Reviewer: - 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

Answer: This argument is intended to compare the theoretical mass flux required to generate the observed convective cooling, and the real estimation of cumulus mass flux. Because our theoretical estimation is only for one convective event, and the cumulus mass flux from previous studies are estimated in the tropical average, we have converted our result to the tropical average. The comparison shows that our result is in the middle of previous estimations.

Reviewer: 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)

Answer: We have modified the sentence to say that air detrains from deep convection is dry when saturated water vapor drops out efficiently during ascending motion.

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