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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

Anonymous Referee #

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Review of ACPD-2004-0175 by Kim Dessler

General Comments

This manuscript is a nice study of the impact of convection on temperatures near the tropopause. The paper addresses relevant scientific questions within the scope of ACP, and is an application of a new data set. The conclusions are relevant and interesting, indicating that convection acts to locally cool the tropopause. The scientific methods and assumptions are generally vaild, but could use some clarification, particularly in the last section where a mass flux estimate has been attempted, but is presented in a confusing manner. The work is sufficiently complete to explain the methodology, and the authors give proper credit to previous work, except they could be more complete at the end of the paper as noted below. The title and abstract are generally clear and concise. The overall presentation is generally clear.

Specific Comments

My major concerns are threefold. First, the daily cooling rate from convection seems not to fit the first figure. Second, there does not seem to be any relationship between the strength of convection and the magnitude of the cold temperature anomalies. Third, the discussion of the global impact of convective cooling through detrainment of low potential temperature air is unclear.

First, the authors sort events by convective onset relative to observation time in figure 2. Where is stage 4 on figure 2? The authors need to explain in a bit more detail how it relates to Figure 1. In the later stages (4-5) the temperature anomalies decay in figure 1, and yet for longer durations the potential temperature anomalies appear larger in figure 2. Where are the events in stages 4-5 in figure 2? Also, it would be useful to put error bars on the symbols in figure 2 so the reader has some sense of the confidence interval here.

Second: Figure 3 could use some error bars as well. This figure shows that for a range of cloud threshold temperatures and fractions, there is no sensitivity in the calculation to the depth of convection or its extent. Is this expected from theory? It seems like there should be some relationship here if the convection is causing the cold temperatures? If not, is it possible that the cold temperatures might actually help cause the convective penetration? In figure 2 the average temperature before convective onset is 1-2K colder than the mean. For example, one likely mechanism is that temperature changes associated with Kelvin waves and the Madden Julian Oscillation might cool the upper troposphere by several degrees kelvin as observed, and the associated instability affects convection. I do not think this can be ruled out from this data, given that there does not seem to be a relationship between the strength of convection and the temperature anomalies.

Third, the discussion on page 7623 to me seems very confusing, and I do not actually follow the math. I appologize in advance to the authors if I have misconstrued their intentions, but the way I would approach the author's calculation is as follows (if I have missed something or this is not what you intended, please clarify this in your revised analysis):

If dr= fraction of detraining air as indicated in the text, then the mixture of air has:

$\theta_m = dr \times \theta_a + (1 - dr) \times \theta$ (where θ_a is the convective air and θ is the environment).

Then: $d\theta = \theta_a - \theta = dr \times (\theta_a - \theta)$ and $dr = d\theta/(\theta_a - \theta)$.

I would not introduce the timescale at all here, because you have had to put it in and then average it out (your 19% -> 44%). I think you can simply use $d\theta = -7.5K$ per day, with $\theta_a - \theta = 20K$ to give $dr = .375$ per day. Also note that 'convection' is misspelled in line 15. The numbers from the analysis above translate to 1.1%/day (0.375 $\times$ 0.03) or an 89 day, (3 month) turnover time.

This is basically the same as your result, but seems to me conceptually simpler and avoids the need for a timescale for convection in the analysis ('dT'). I'm not sure if you intended dr to be a ratio? If I've missed something (which is entirely possible), please correct me, and the manuscript.

Finally, the subsequent analysis here could be clarified, and should cite some additional work. You should probably note that this is a lower limit on the convective turnover time, because it assumes the direct injection of air is responsible for all of the observed cooling, which is likely not the case. For example, from figures 1 & 2, the pre-convective environment in stage 1 is 1-2 K colder than the mean. Also, it is likely that radiative cooling from anvil clouds, cirrus clouds and the large scale dynamic response to convection has some impact (as indicated by Sherwood (2003)).

It would be useful to note also the turnover time / mass flux estimates of Küpper et al

(2004) and Gettelman et al (2002) here as well. Given the level of 208K for the clouds (something like 14.5km or lower if the clouds are cooler than their environment), your estimates appear to be within the range of previous work. It looks like perhaps your estimates are on the high side of a mass flux, or the short side of a timescale, which would be expected from the method.

On balance, with some better discussion and clarification as indicated above, this paper will make a valuable contribution to the literature.

Reference (others in the manuscript): Küpper, C., J. Thuburn, G. C. Craig, and T. Birner (2004), Mass and water transport into the tropical stratosphere: A cloud resolving simulation, J. Geophys. Res., 109, D10111, doi:10.1029/2004JD004541

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