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Interactive Comment

Interactive comment on "Cirrus cloud-temperature interactions in the tropical tropopause layer: a case study" *by* J. R. Taylor et al.

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The authors wish to thank Prof. Durran for his valuable comments and feedback on the manuscript entitled Cirrus cloud-temperature interactions in the tropical tropopause layer: a case study that is currently being considered for publication in Atmospheric Chemistry and Physics. To the best of our effort, we have endeavoured to address each of the his suggestions. Below is a detailed account of how this was done. We welcome any further feedback that the editor and/or reviewers may have.

1) One major concern of the authors, that "the temperature observations do not show any indication of the expected infrared heating" appears to be misplaced. The 2D simulations of the response to a fixed heat source similar in shape to thin cirrus in Durran et al. 2008 show that only a small part of the potential heating is actually

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realized as a temperature rise. In the case shown in Fig. 3b of that paper, a fixed heat source of 3 K/day produces a temperature rise of less than 1.5 K in 24 hours and that temperature rise asymptotes to only 1.85 K at infinite time. Moreover, the 2D geometry greatly overestimates the ultimate (although not the initial) temperature rise, which in 3D is zero and therefore in perfect agreement with the authors' results. The only reference I know for this is Bretherton and Smolarkiewicz (1989), see the paragraph into which eqns (9)-(11) are embedded.

The 2D calculations in Durran, et al. show that approximately half of the fixed heating gets translated into a positive temperature perturbation (that is, 3 K/day heating results in 1.5 K increase in a day). The COSMIC temperature observations do not show any such positive change in temperature that would be associated with the expected IR heating of the cloud. The discussion in Bretherton and Smolarkiewicz is based on a radially symmetric realization of a 2D convective-adjustment tropospheric model in which they observe no apparent temperature perturbation associated with heating because all of the associated energy appears to be conserved in buoyancy displacements. Arguments addressing the validity of this model aside, we do not observe any signatures that are indicative of large scale buoyancy perturbations, but the temporal dispersion of the observations may inhibit our ability to do so. To ensure that we are including this potential hypothesis, we have added the following text to the discussion: "There has been a previous attempt to model fixed heating with no resulting temperature perturbation [Bretherton and Smolarkiewicz, 1989], but this yielded extensive buoyancy displacements and no such signatures are apparent in the COSMIC temperature field."

2) p. 5, 2nd paragraph: while convection can certainly lead to vertical motion, it is hard to see how such convection would sustain a cloud of this very large horizontal scale. On the other hand, heating in a stratified fluid can produce updrafts on the scale of the entire cloud and, unlike buoyant convection, this mechanism does not require weak static stability. The authors discuss Dinh et al, 2010, and I find Fig. 7 of that paper to be surprisingly similar to Fig. 4 in this manuscript. The Dinh case is certainly idealized,

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but not unreasonably far from scenarios considered in this paper (r= 3 microns, N=1.3 cmËĘ(-3), cloud depth 500 m in Dinh, whereas r=5.6 microns, N=1 cmËĘ(-3), with the same 500 m cloud depth in last case in Table 2.) Perhaps this event is an example of IR/gravity-wave-driven ascent of very small ice crystals?

While we appreciate that our Figure 4 is visually very similar to that of Fig. 7 in Dinh, et al., we are hesitant to argue that this is indeed evidence of gravity-wave-driven ascent of very small ice crystals. The ice crystal radii investigated in Dinh, et al., are considered unrealistically small throughout the lifetime of the cloud; in 48 hours, the mean radius never exceeds 4 microns. One of the hypotheses that we entertain in the discussion does address the possibility that exceptionally small ice-crystals are created through rapid freezing, but our calculations show that our best estimates still have ice crystals with radii almost twice the size of that found in Dinh, et al., (5.6 microns compared with 3 microns). However, so as not to exclude this possibility, we have added the following text: "This is consistent with the gravity-wave-driven ascent hypothesis for cloud maintenance proposed by Dinh, et al., [2011] but it is unlikely that these ice crystals are as small as those used in their model."

3) p. 4, 2nd full paragraph: The temperature anomaly in Fig. 7 features cold over warm, which would correspond to a anticyclonic PV anomaly. However, if this is an intrusion that has been isentropically advected into the tropics from the subtropics, one would expect the PV anomaly to be cyclonic.

This is an important point and we thank the reviewer for highlighting this. To adequately clarify confusion about the behavior of PV during this time, we have added a new figure to the manuscript (referred to as Figure 9 and appended below) and modified the final two paragraphs of the discussion to read: "The longitudinal distribution of daily-mean temperature anomalies on 28 January (Fig. 7) confirms the localized nature of this anomaly. The cold anomaly is restricted to the Eastern Pacific region with a warm anomaly present directly below. This vertical temperature structure can arise from balanced flow about an isolated (anti-cyclonic) potential vorticity anomaly (Hoskins et al.,

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1985). The horizontal structure of temperature anomalies at 16 km over 27–29 January is shown in Fig. 8. This structure highlights that the cold temperature anomalies in the Eastern Pacific (90_W to 130_W) are linked with patterns in the subtropics; note the minimum near 15_S). This behavior is distinct from equatorially-centered temperature anomalies linked to equatorially-trapped waves, such as the Kelvin wave pattern seen over longitudes 0_–120_ E in Figs. 7–8 (e.g. Randel and Wu, 2005). The overall latitudinal and vertical structure of the Eastern Pacific temperature anomaly is more consistent with dynamical features originating in extratropics, extending into tropical latitudes, such as are often observed in the Eastern Pacific (Waugh and Polvani, 2000). The dynamical behavior during January 2009 is illustrated via the potential vorticity (PV) maps for the upper troposphere (200 hPa) derived from NCEP reanalysis shown in Fig. 9, for time periods prior to (January 10-23) and during (January 27-29) the cloud event. The period of January 10-23 (Fig. 9a) was characterized by persistent cyclonic PV patterns extending into the tropics in the Eastern Pacific from both the Northern Hemisphere (NH) and Southern Hemisphere (SH) (associated with an equatorially-centered westerly wind maximum in this region). This structure changed over a few days during ∼January 24-28, wherein the SH intrusion moved westward and extended across the equator into the NH, and the NH intrusion extended into the SH (Fig. 9b). In the Eastern Pacific region of the cirrus cloud (boxed region in Fig. 9), the resulting PV anomalies for January 27-29 were anti-cyclonic compared to the persistent pattern earlier in the month, consistent with the balanced temperature structure of warm anomalies in the troposphere and cold anomalies in the lower stratosphere (seen in Fig. 7). Hence the cold temperatures and cloud formation occurred in concert with the dynamical evolution seen in Fig. 9.

The circulation in the region of the cloud was studied based on NCAR reanalysis winds. The evolution of 150 hPa winds (the approximate pressure level of the cloud) during the six day cold period indicate that this region was relatively stagnant at this time. Calculated trajectories of air parcels within the cloud observed on 28 January 2009 (see Fig. 1) were seen to only move _200 km over 2 days. The only fresh air that comes into this region is at the extreme southern end of the cloud; south of _17_ S. In Fig. 1, this portion of the cloud is somewhat different from the rest of the cloud, possibly due to the in-mixing of fresh air."

4) p. 3, first paragraph, righthand column: I'm not sure exactly what the authors intend to say, but "Figure 2 does not show evidence of coherent small-scale spatial structure (e.g., wave-like activity) embedded within the 3000 km cirrus cloud)" does not seem to allow for what appear to be a pair of 500-km wavelength waves centered around tic-mark label 3000.

With this statement, we were attempting to explain that there is no coherent wave pattern throughout the length of the entire cloud. The reviewer has identified a small structure attached to the trailing end of the cloud which we feel is not indicative of the overall cloud behavior. To clarify our statement, we have modified our sentence to read: "Figure 2 does not show evidence of coherent small-scale spatial structure (e.g. wave-like activity) embedded throughout the entire 3000 km cirrus cloud"

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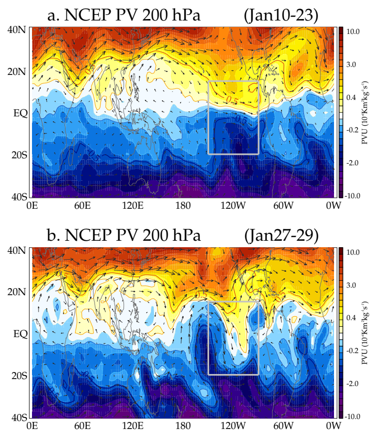

Fig. 1.

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