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Interactive comment on "A redistribution of water due to pileus cloud formation near the tropopause" *by* T. J. Garrett et al.

Anonymous Referee #1

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This manuscript presents observations and parcel-model simulations of pileus clouds near the tropical tropopause. Although, pileus are perhaps worthy of study and potentially important for dehydration of the tropical tropopause layer, this manuscript does not really provide any new information about these clouds. As described in detail below, in several instances the observations do not support the conclusions drawn by the authors. In particular, the authors assert that the CRYSTAL-FACE measurements indicate mixing of pileus and convective cloud, but the measurements are also consistent with diluted convective injection directly into the tropopause region. Much of the analysis is insufficiently described, and uncertainties are not acknowledged. Unless these flaws are addressed, I cannot recommend publication of this manuscript in ACP.

Specific Comments:

1. Page 8211, lines 16-18: The authors suggest that the photographed pileus extended about 2 km into the lower stratosphere. It is implausible that the pileus formed in the arid lower stratosphere, and such an assertion certainly cannot be made based on a photograph.

2. Page 8212-8213, lines 6-8: The authors suggest that "much of the TTL cirrus observed during CRYSTAL-FACE originated as pileus that had mixed with the deep convection." The fact that tracers indicated a mixture of TTL air and convective outflow does not necessarily imply that pileus were involved. The TTL cirrus cloud simply have been diluted convective outflow into the TTL.

3. Page 8213, last paragraph: The delta-HDO observations shown definitely do not imply that the TTL cirrus was a mixture of pileus and convective injection. TTL cirrus formed directly from anvil ejecta would not necessarily have delta-HDO of zero because significant entrainment occurs in the convective updrafts.

4. Page 8215, lines18-19: The authors should acknowledge that since they are using a parcel model, sedimentation cannot be treated. Hence, the important issue of dehydration by the pileus cannot be investigated.

5. Page 8216, lines 4-7: As demonstrated by Haag and Kaercher [ACP, 2, 1467-1508, 2002], under rapid cooling, relatively large aerosols can get far out of equilibrium. They can't take up water rapidly enough and remain concentrated, thus delaying freezing. The authors should qualify their statement here.

6. Page 8216: Is the Kelvin effect included for calculation of aerosol concentration and freezing? This effect can be important for the small aerosols.

7. Pages 8217-8218: The discussion of the gravity wave analysis is extremely unclear, and key assumptions are not stated. For example, the authors are implicitly assuming that the wave was forced by the convection acting as an obstacle to the flow. How do

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you know the forcing level is at 15 km, and what is a "forcing orthogonal altitude"? How was the wave frequency determined? The authors use the pressure-weighted mean wind (surface to tropopause) as a measure of the convection velocity. Isn't it more appropriate to use the steering-level wind at 700 mbar? The apparent horizontal wavelength is 100 s x 193 m/s = 19.3 km. The authors are apparently using a geometrical argument to get 7.3 km based on assumptions about the speed and direction of the wind at the forcing level as well as the speed and direction of the convective plume. Where does the 4.2 km wavelength come from? All of this should be explained more clearly, perhaps with a diagram.

8. Perhaps the gravity-wave analysis should be omitted altogether. Apparently the only point of it is to come up with an amplitude and frequency to force the parcel model simulation. These estimates are highly uncertain, and the only data the model results are compared with is effective radius. Given the uncertainties in the wave forcing, the unknown mixing between pileus and convective cloud, and the large uncertainties in effective radius measurements for such small crystals, the claim of confirming the conceptual model is very dubious.

9. Page 8219: The discussion of the parcel model results is very brief. More details should be provided. For example, the effective radius for the pileus of 1 micron is surprisingly small. What were the ice crystal concentration and ice water content in the cloud?

10. Page 8220, lines 13-16: The authors state that the MLS data indicate that the lower part of the TTL is frequently supersaturated, but the upper part is rarely supersaturated. The low frequency of supersaturation near the tropopause in the MLS data is probably a result of the poor vertical resolution of the measurement (3-4 km). They are no doubt averaging very dry stratospheric air with humid air near the tropopause. In situ measurements generally indicate that supersaturation is most common near the cold-point (i.e., the top of the TTL).

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11. Page 8220, lines 19-21: The authors use frequency distribution of ice saturation ratio from MLS along with an estimate of the lifting distance required for initiation of ice nucleation to calculate the frequency of pileus formation in the tropics. However, as has been demonstrated in previous studies (e.g., Jensen et al., GRL, 1999), MLS data cannot be used as a measure of the frequency of supersaturation in the upper troposphere. The breadth of the MLS relative humidity frequency distribution is dominated by the poor precision of the individual MLS measurements. As a result, the fraction of observations indicating supersaturation are also dominated by the precision problem. In other words, the width of the distribution does not necessarily represent the geophysical variability.

12. Page 8220, lines 26-27: "Finally, aircraft measurements off Honduras and Costa Rica show air mostly supersaturated in the middle TTL (Fig. 7, Jensen et al., 2005)." (1) Jensen et al. showed one profile of aircraft data indicating supersaturation at the cold point tropopause (upper TTL); (2) Figure 7 of that paper shows *model results* at of water vapor mixing ratio at 380 K (upper TTL) and cloud frequency - not supersaturation.

13. Page 8221, line 4: "lifetimes under gravitational settling is of order 10 days." What vertical distance are you using for this estimate?

14. Page 8221, lines 9-11: Again, one cannot distinguish cloud initiated as pileus from ice detrained directly from convection based on the aircraft measurements.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 8209, 2005.