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Atmospheric Chemistry and Physics Discussions

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

T. J. Garrett et al.

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We thank the reviewer for their comments, as they have helped us improve and clarify the manuscript.

1. Pileus form from lifting, so certainly it is plausible that moist tropopause air could be lifted into the stratosphere by convection. We estimated the altitude based on the radiosonde determined tropopause height. It was assumed that the height of the convection, at a time so close to extremely high precipitation, was indicative of the height of the tropopause. By extrapolation of the height on the photo taken up by the cloud, it was estimated that the pileus cloud was 2 km above. Certainly this seems extreme, but then so was the cloud. As stated in the text,

"it is impossible to say with certainty", but the estimates are plausible. However, to limit speculation, we have omitted reference to the stratosphere in the revised discussion.

2. There are two other bits of evidence mentioned in this paragraph that the reviewer omitted. First, a monochromatic wave signature was seen, and that the tropopause cirrus layer was vertically separate from an anvil layer beneath, which suggests that something other than simple mixing was going on. We discuss this issue in greater detail now in the revised text. In particular, we show that if simple mixing were involved, TTL cirrus crystals would be expected to be the same size or larger than ice crystals in deep convection at the same level. For the case shown in Fig. 4, the opposite was observed, a fact that suggests that the mixing was not with initially clear supersaturated air, but rather with cloud with extremely small ice crystals. Mixing with pileus cloud is both plausible and consistent with the observations.

3. We now limit discussion to how this figure indicates a role for mixing in the formation of TTL cirrus.

4. It is not quite true to state the precipitation rates can not be estimated with a parcel model. If this size of the crystals is known, an estimate can be made of their settling velocity. Based on an assumed layer depth (this assumption ostensibly being the problem with the parcel model), a rate of dessication can be estimated. But, in any case, we deliberately restrict our discussion to description of the phenomenon as it pertains to formation of condensate in the TTL. Whether such condensation ultimately leads to dehydration is beyond the scope of this paper.

5. Unfortunately, we were unable to find the paper refered to as listed by the reviewer, or in a number of possible permutations. However, for the physical reasons described in the paper, we believe that something must be wrong with the

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argument that haze aerosol are able to get out of equilibrium with their environment. Simple scale analysis shows that the time scales associated with diffusion of water vapor to the surface of an aerosol are simply far too short not to adjust to ambient atmospheric cooling, no matter how rapid.

6. Yes, the Kelvin effect is included. The effect on aerosol equilibrium activity is no more that 10%, even for the smallest aerosol modeled. The text now makes reference to this.

7. We appreciate these suggestions. We have moved the description to an appendix, added a diagram, and attempted to clarify our reasoning.

8. Our justification for the quantity of mixing is based on total water mixing ratios in the convection and surrounding TTL shown in Fig. 4. We have added a new set of measurements to bolster the effective radius calculations, explained that they are consistent with others calculations, and have described in more detail why we think this is an important measurement for indicating the cloud processes involved.

9. Yes, we were surprised by how small the ice crystals were too, but the results are consistent with those described by Karcher and Lohmann 2002b for the sorts of updraft speeds used to initialize the model. We now include more detail on the concentrations and mixing ratio in the cloud.

10. We fully agree with the argument that the MLS data underestimates the incidence of high humidity layers. In fact we say so, and back this up by including aircraft profiles from CRYSTAL-FACE and the reference to the Jensen et al. (2005) paper.

11. We acknowledge the precision problem of the MLS, and this is the reason why we include some aircraft data. We are a bit limited here until HRDLS data are

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released, and unfortunately even these may not prove much improvement given its difficulties.

12. Some unfortunate confusion here. We have changed the text to read “Higher resolution aircraft measurements off Honduras and Costa Rica show air mostly supersaturated into the middle TTL (Fig. 10). Similar results have been noted in TTL air near Costa Rica (Jensen et al., 2005).

13. We have clarified the statement to read “settle gravitiationally several tens of meters per day”

14. We have tried to argue more clearly what we are trying to show, which is that photographs indicate pileus exist in the TTL. Convective formation of pileus, and subsequent mixing, is the mechanism most consistent among several with the aircraft measurements obtained in TTL cirrus of Honduras.

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