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Interactive comment on "The impact of cirrus clouds on tropical troposphere-to-stratosphere transport" *by* T. Corti et al.

Anonymous Referee #2

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General Comment:

This paper goes to some length to calculate cloud radiative heating in the tropics, in a one-dimensional context. I think these complex and sophisticated calculations do indeed demonstrate that cloud radiative heating is a significant diabatic forcing in the TTL. However, the paper would not be publishable on these grounds alone, because the cloud radiative heating calculations in this paper appear to be improvements on previous calculations in their GRL, 32, L06802. (For example, Figure 4 of this paper is similar to Figure 3 of the GRL.) This paper also proposes a new transport mechanism by which air is transported through the TTL to the tropopause. In this respect, the arguments are overly simplified (details below). Before publication, it would be essential that the authors demonstrate the validity of their transport model against available chemical tracer, or other independent evidence.

Specific Comments:

Page 1729, lines 10 - 22. I don't understand why the existence of small scale variability in heating rates (associated with clouds or temperature variations) should undermine the idea that the net large scale upward mass flux in the TTL is controlled by wave breaking ("downward control"). I don't think any proponents of downward control would argue that small scale variability does not exist, does not give rise to regional differences in upwelling, and associated induced horizontal circulations, so I don't see why the existence of this variability is inconsistent with downward control. The authors claim that "radiative heating in cirrus clouds may translate into a larger upwelling". Why? In principle, one could attempt to establish this in a three-dimensional interactive model in which one added radiative heating clouds. But that is not done here. The usual argument would be that increased cloud radiative heating would reduce the need for clear sky radiative heating, so for fixed externally imposed upwelling mass flux, would warm temperatures in the clear sky. Much of this discussion seems to confuse forcing on large and small scales and should be removed.

p. 1727: "a combination of deep convection reaching close to Q = 0 with subsequent large scale upwelling in clear sky was too slow to explain the time lag." In following Sherwood and Dessler here, the authors are setting up a straw man. It is much more likely that there is some distribution of convective outflow above the Q = 0 level, which tails off as the cold point tropopause is approached. It is not reasonable to propose some finite detrainment at the Q = 0 level, and zero above it (i.e. a detrainment profile with a singularity). Why should a detrainment profile suddenly go to zero? It is certainly likely that cloud radiative heating shifts a continuous cloud detrainment profile higher than it would otherwise be. But air parcels with 365 K in equivalent potential temperature do exist in the tropical boundary layer, so convective outflow above 360 K is theoretically possible without cloud radiative heating. Since the BD circulation is

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about 100 times smaller than the Hadley circulation, one could argue that the near surface air parcels most likely to detrain in the TTL are those at the highest 1 percentile of equivalent potential temperature.

P. 1727, line 26. I don't think anyone would have ever claimed the tropical tropopause was a material surface, in the sense of inhibiting upward transport across it. Otherwise, how would tropospheric air ever enter the stratosphere?

p. 1737, the authors seem to be defining the "convective" mass flux as any mass flux that occurs within clouds that is not associated with radiative heating. Is this correct? It should be defined. The term "without the influence of deep convection" is confusing since most of the thick stratiform anvils from which most of the cloud radiative heating would arise, would be associated with deep convection.

Figure 4, and other places. "The black line shows the full sky mass flux ..". It is important for the authors to remind the readers that this is a full sky "radiative" mass flux only. Below the TTL, the net upward mass flux in clouds is canceled by the net downward mass flux in clear sky regions (to the extent that the tropics can be regarded as a closed circulation), so the full sky mass flux must be near zero.

The Gettelman mass flux would presumably be an estimate of the mass flux from clouds in which cloud radiative heating is included. One can, as done in this paper, make a conceptual separation of the cloud mass flux into "convective" and "radiative" components, but how would one remove the "radiative" component to get the "convective" component when trying to diagnose the cloud mass flux from satellite imagery? i.e. from the outside, how would you determine if a mass flux is driven by radiative heating or some other diabatic process? i.e., what is the justification for referring to the Gettelman mass flux as "convective" rather than "total cloud = convective + radiative"? The authors should attempt to justify this terminology.

Bottom page 1736 and Conclusions. The authors suggest that part of the convective outflow remains within a cirrus cloud for 15 days, is continuously exposed to cloud

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radiative heating for these 15 days, and so that these air parcel ultimately detrain at 370 K. This seems highly unlikely. What is the justification for this assumption? Most clouds continuously process air that flows through them, so the typical residence time must be much less than 15 days. It would be much more reasonable to argue that cloud radiative heating shifts the convective outflow profile of detraining air upward by up to several K in potential temperature, rather than to say that its effects are exclusively concentrated in a tiny fraction of air parcels. This seems to be related to a simplified picture of convective outflow ("straw man picture") discussed above, rather than seeing a detrainment profile as a continuous distribution.

In this context, I think it is extremely important that the authors incorporate the effects of cloud radiative into a transport model which makes specific, testable predictions on chemical tracers, and which does not rely on the ad hoc assumptions discussed above that give rise to a single age of 15 days at 370 K. i.e. this model would allow for a continuum of outflow heights, and consequently a continuous distribution of air parcel ages at the tropical tropopause.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1725, 2006.