

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

T. Corti et al.

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Answer to General Comment: In the general comment, the referee judges the manuscript not to be publishable because it appears “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)”. We disapprove strongly. Our GRL paper has dealt with the radiative energy balance and derived vertical mass fluxes. The transport mechanism proposed in the present manuscript has not even been mentioned in the previous paper. As for Figure 4 in our manuscript, the “in cloud” radiative mass flux shown in the blue curve is a central point of this manuscript and has not been shown in our GRL paper, or in any other publication. Therefore, the GRL paper and the present manuscript differ considerably concerning their scientific message.

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Furthermore, the referee judges our arguments to be “overly simplified”, stating in more details in the specific comments that calculations with a transport model would be important. We agree that further investigations will be required to corroborate the conclusions drawn here. Investigations with transport models are under way and will go beyond the scope of this paper. However, until they become available, we maintain that our paper, including its “complex and sophisticated calculations” (cit. referee), makes an important point. In addition, we have demonstrated the plausibility of our transport mechanism by comparing expected transport time scales with those derived from chemical tracer measurements.

Answer to Specific Comments: Page 1729, line 10-22: The referee is wondering why the existence of small scale variability in heating rates should undermine the idea of ‘downward control’. Well, it does not. We are not aiming at undermining the idea that the net large scale upwelling is controlled by the downward control principle. The point we are trying to make is that small scale variability gives rise to local differences in upwelling. The referee obviously agrees with this statement and believes it well accepted in atmospheric science. We know however from our own experience that some atmospheric scientists believe that small scale variability in heating rates leads to variability in temperature only, assuming that the upwelling velocity is determined by the downward control principle alone. We realise that our formulation “radiative heating in cirrus clouds may translate into larger upwelling” is confusing and have replaced it by “radiative heating in cirrus clouds translates locally into faster upwelling than in cloud free air”. We also clarified the formulation in the summary to section 2, replacing “driving the tropical upwelling on the mesoscale” by “determining the velocity of tropical upwelling on the mesoscale”.

Page 1727: The referee criticizes the transport mechanism discussed in Sherwood and Dessler, 2003, arguing that is not reasonable to assume a convective detrainment profile with a “singularity” at the $Q = 0$ level. (We assume that the referee was referring to a “discontinuity”, not to a “singularity”.) However, neither Sherwood and Dessler nor

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we are claiming that there is a discontinuity in convective detrainment at the level of $Q = 0$. The mechanism discussed in this part of the paper consists of convective transport reaching close to the level of $Q = 0$ and subsequent radiative heating in clear sky. Sherwood and Dessler have clearly demonstrated in their paper that this mechanism (which had previously been proposed as an explanation for the troposphere-to-stratosphere transport) will lead to longer transport rates than observed, and we refer to their conclusion.

The referee writes that “convective outflow above 360K is theoretically possible without cloud radiative heating”. We agree and are already saying the same in our ACPD manuscript from page 1726, line 25 to page 1727, line 2. Moreover, the referee states “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”. We agree, one could argue in this way. But we cannot see that this would lead anywhere. In our study, we compare radiative mass flux with convective mass fluxes from a quantitative analysis of detrainment rates by Gettelman et al. (2002). We believe that this is a better way forward than theoretical considerations.

We think that our discussion of the transport mechanisms is clear enough in the existing version of the manuscript.

Page 1727, line 26: In our manuscript, we are not at all claiming that “anyone would have ever claimed the tropical tropopause was a material surface” (cit. referee). All we are saying is that the fact that the tropical tropopause is not a material surface has led to the introduction of the tropical tropopause layer (TTL). The statement in our manuscript is correct and justified as we want to mention the motivation for introducing the TTL.

Page 1737: The definition of “convective transport” is already included in the original manuscript on page 1728, line 17 - 19. We agree that the term “without the influence of deep convection” in line 14-15 might be misinterpreted and use “without the release

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of latent heat release”. We thank the referee for spotting this lack of clarity.

Figure 4 and other places: We agree with the referee that the readers should be reminded that we present “radiative” mass fluxes. We have done the appropriate changes in the revised manuscript.

Gettelman mass flux: The referee suspects that the mass flux estimated by Gettelman et al. (2002) would be an estimate including cloud radiative heating. This is clearly not the case. In Gettelman et al., 2002, the mass flux is called “convective flux” and is derived from satellite images on a 3-hour time scale. Convective mass fluxes are clearly dominated by latent heat release and mixing processes, whereas radiative processes play a minor role. The radiative mass fluxes presented in our manuscript originate mainly from thin or even subvisual cirrus clouds which have not been considered in Gettelman et al. (2002).

Bottom page 1736 and conclusions: The referee criticizes the assumption that part of the convective outflow remains in a cirrus cloud for 15 days. We realize that our formulation in the conclusions suggests this literally and that we have to improve this. The main point is that our calculations show that convective outflow, which stays inside cirrus cloud for most of the time, can reach potential temperatures of around 370 K in two weeks. The assumption that there air masses staying more or less inside cirrus clouds a longer time is supported by the finding that cloud lofting has a favourable effect on the cirrus cloud lifetime (Lilly, 1988). We still think that upward transport in cirrus cloud from 350 to 370 K in two weeks is realistic. We state this more carefully in the revised manuscript. Furthermore, we mention now specifically in the paper that further investigations are needed to clarify the influence of cloud lofting on the cirrus cloud lifetime. We thank the referee for pointing out this critical issue.

Transport model: The referee suggests the incorporation of cloud radiative interaction into a transport model. We appreciate this suggestion and agree that this is (as we state in the manuscript) an important next step. However, this goes far be-

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yond the scope of the present paper. Our manuscript provides a plausible explanation of troposphere-to-stratosphere transport, supported by quantitative calculations. We strongly believe that these calculations are interesting and important enough to justify their publication.

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

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