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Interactive comment on "Transport across the tropical tropopause layer and convection" *by* A.-S. Tissier and B. Legras

Anonymous Referee #1

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This paper discusses the convective sources of air entering the stratosphere in the tropics, examining the variation throughout the year as well as the contributions from specific convective regions. A number of sensitivity studies are conducted, including sensitivity to the details of the convection (satellite data treatment) and the analysis dataset used.

General comment:

If this were the first paper examining the issue of convective sources for air in the tropical lower stratosphere, this paper, with suitable edits, would be fairly reasonable as is. However, it is not the first paper, and the results differ markedly from previous work (to which the authors refer) by Bergman et al (2012). The differences start with Figure

2, where there is no seasonal variation in what fraction of parcels reach convection from 380K in the authors' results, whereas the previous work indicates a marked seasonal variation, with a far lower fraction reaching convection in boreal summer than boreal winter (60% boreal winter, 15% boreal summer, vs 85% constant all year for the present work). (I doubt that the difference in trajectory length – 60 days in Bergman et al and 90 days for the time window of Figure 2 – makes a great deal of difference).

The reason for the disagreement is no mystery. Diabatic heating rates from the ERA-Interim analysis (and probably from the JRA analysis as well) are very different in boreal summer (perhaps boreal winter as well) from the observations used in Bergman et al. Comparing Figure 6 of Bergman et al, LZRH's vary from 359 to 374K in Boreal summer for the observationally based heating rates, whereas ERA-Interim analysis LZRH's from 350 to 367 (Figure 3).

I am not insisting that Bergman et al are correct and that the authors are wrong. It could well be the other way around. My concern is that someone not familiar with the literature will read this paper and come away with the sense that this problem is much more settled than it really is. This is not intended as a review paper, but an important part of science is reproducing experiments. If the results are different from a previous experiment, we need to understand why they are different, and have some discussion of who is right, and why. The fact is, that these results are very sensitive to the distribution of diabatic heating, and far too little attention is devoted to this issue in the paper.

A comprehensive survey and evaluation of radiative heating rates (and their seasonal and geographic variation) is beyond the scope of this paper, but showing some maps of the heating rates and vertical profiles (both JRA and ERA-Interim), as well as references to other methods (e.g., the Bergman et al paper) is called for. I think some defense of why the authors' analysis-based (as opposed to observationally based) diabatic heating approach is superior is also called for.

Specific comments:

Page 26240, line 8: It would be useful to explain, however briefly, how any convection (say, in the summer) with a cloud top theta of 348 can make a contribution to transport at 380K. Presumably, some of these 348K "emitted" parcels move horizontally into a region where the LZRH is less than 348K, and rise upward. It is not clear why the Tibetan high efficiency is a paradox. Based on the distribution of colored arrows, most heating rates in the tropics in the ERA-Interim analysis are positive at the Tibetan mode max of 365K (even if they are negative at 365K over Tibet). So, a little horizontal wandering will get the parcel into positive heating rates. Since the parcels are already so high, getting to 380K should be easier than for parcels that start at a lower theta (convective emission theta). In general, a little more mechanistic explanation of the findings in the figures would be helpful.

Figure 5:

I understand the comparison between the backward trajectories for the two different satellite cloud altitude treatments (1 km offset), but am confused by the drastic difference in the forward trajectories. With lower clouds, it takes longer for emitted parcels to get to 380K, so is it a matter of integration time in the forward direction? 3 months (backward trajectories) is still enough time for parcels from either the low or high cloud case to mostly (80 plus %) get to 380K. More explanation is needed here.

I assume the LZRH in Figure 3 for the different sources represents a mean LZRH for the locations of convection in that season and region (AML, SAP...). Might be good to make this clear. The LZRH must jump around a lot, because a lot of convective systems in the AML in DJF (for example) with cloud tops much less than 359K contribute to air at 380K.

There is no reference to the results of section 5 in the abstract that I can see.

Minor corrections:

P. 26233, line 29: "..of our results TO uncertainties " P. 26233, line 19: "..during THE Asian monsoon..." this error occurs elsewhere P. 26236, line 24: "..Sea of China and Tibet defined..." P. 26238, line 29: I think you mean Asian land convection fading OUT, rather than IN. How about: "when Asian land convection subsides..." P. 26242, line 27: "This modification enlarges high..." P. 26245, line 1: "Another, more traditional.." P. 26250, line 19: "meet out conclusions??" What does this mean? P. 26250, line 27: "Separating the location of convection from the entry point into the..."

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Interactive Comment