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Atmospheric Chemistry and Physics
Discussions

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

A.-S. Tissier and B. Legras

as.tissier@lmd.ens.fr

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We thank referee #2 for his thorough reading of our manuscript, his comments and his numerous corrections of our poor English. We answer below only to the comments about the scientific content.

Answers to general comment

1. ***The current title is rather vague and, while accurate in a broad sense, does not properly convey the content of the manuscript. The problem is mostly in the order of the words; for example, something like "Convective sources***

of trajecto- ries traversing the tropical tropopause layer" or "Convective sources of trajecto- ries entering the tropical stratosphere" would be better. The abstract is concise and complete, but should be edited to improve the clarity.

   The title has been changed : "Transport across the tropical tropopause layer and convection" → "Convective sources of trajectories traversing the tropical tropopause layer" following the suggestion of the referee. We have edited the abstract.

2. ***As the CLAUS record has been extended to provide more overlap with the CALIPSO cloud lidar, I wonder if anyone has tried to collocate these observations to assess the actual biases in cloud top pressure derived from CLAUS. This could be done by, e.g., treating the CALIPSO track like a trajectory (which would also incorporate and be used to test interpolation-related uncertainties). Minnis et al. (2008) did a similar analysis for MODIS-derived cloud tops. One could imagine that this could quantitatively (and even qualitatively) impact estimates of the sensitivity of the source distribution (and particularly transit times) to the corrected cloud tops, as the implied correction could depend on the derived cloud top pressure and/or have regional variations. Given that the CLAUS brightness temperatures are often used in this way and such a specialized analysis would not stand on its own, I would really like to see someone provide this in one of these studies. Although I feel that it would be appropriate here, I propose this as a suggestion rather than a requirement.***

   The CLAUS dataset contains brightness temperature at 10.8µm from geostationary satellites and our derivation of the cloud top height is basically identical, for high opaque clouds, to the infrared radiance method used in algorithms deriving cloud top height from broadband infrared imagers. Kwon et al. (2010) and Hamann et al. (2014), both using SEVIRI data and comparing the derived cloud top height with CALIPSO and CLOUDSAT reach conclusions which are consistant with the earlier study of Sherwood et al. (2004) and our approximation. Such studies extend naturally to CLAUS data.

Answers to specific comments

1. ***In Fig. 2, lines in the same plot are shown on the same axis, but with that axis representing different aspects of trajectory time (tropospheric source vs stratospheric receptor). Although I don't disagree with the rationale, this choice of presentation is potentially confusing. Given the lack of major month-to-month variability in the proportion of trajectories reaching a cold cloud top, I think that the first result (maximum of 88.7% in April; minimum of 84.7% in July) could simply be reported in the text without graphical representation.***

   We have modified the caption to avoid the confusion but we think it is useful to keep this curve as the stability of this proportion throughout the year is an important result which differs from the previous study of Bergman et al. (2012).

2. ***It is not paradoxical that transport from convective sources over Tibet is particularly efficient even though the LZRH is high (p.26240, l.11–12) when one considers that the prevailing winds within the anticyclone will tend to isentropically lift these trajectories above the LZRH over the subcontinent.***

   Actually some cross-isentropic motion is necessary to reach the 380 K surface even if the high level of sources above the Tibetan plateau requires less heating than for other regions to perform that task. The main effect of the anticyclone is to confine the air from the Tibetan plateau within its core with limited meridional displacement. The lifting effect of the anticyclone is much stronger for the air originating from regions south of the Tibetan plateau and ventilated by the easterly

   branch of the anticyclone. We will discuss those aspects in a second article in progress that focuses on the horizontal displacements. We agree that the results about Tibet are not paradoxical and we have modified the sentence.

3. ***Note that one of the key differences among the studies cited on p.26250, l.16–19 is the choice of reanalysis/analysis data (as well as the representation of convection). In particular, all of the studies identified as agreeing with these results are based on ERA-Interim. Although this is only one region, this reinforces the point that the results may be more sensitive to the choice of reanalysis than indicated by the comparison of ERA-Interim and JRA-55. It's not necessary to provide more sensitivity studies here, but section 4.3 should also include relevant contextual information from previously published results.***

   We have added a comparison to the MERRA reanalysis data and a discussion on the differences between our result and those of Bergman et al. (2012).

Answers to technical suggestions

We have taken into account the minor technical suggestions not mentioned below.

p.26248, l9-10: I'm not sure I quite follow this statement âĂŤ is the source curve in Fig. 2a supposed to be the sum of the individual regions (which are arranged relative to intersection with convection) or the total number of trajectories coming from convection (which are arranged relative to release at 380 K***)? Is the shift a lead or a lag? This sentence should be clarified.***

The discussion has been clarified. We now discuss the regional curves and the shift is a lag (in normal time running forward). This is expected since a parcel leaving a cloud reaches the 380 K surface after a transit time.

p.26267, figure : recommend reordering the panels so that season is top/bottom and category is left/right – although this makes it more difficult to evaluate the relative locations of the modal peaks, it makes it easier to compare the distributions (i.e., redistribution among regions) and would be consistent with the previous figures (thereby limiting confusion)

We understand this point but we have added two more panels to this figure ("smoothed heating rates") and this layout makes easier to compare the relative locations of the modal peaks.

p.26268, figure : it may be worth repeating the upper panels of Fig. 7 and showing this as a four-panel figure to facilitate comparison

We followed this advice and have also added two panels showing the vertical source distribution obtained with MERRA reanalysis.

References

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C10720/2015/acpd-15-C10720-2015-supplement.pdf

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