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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A Lagrangian view of convective sources for transport of air across the Tropical Tropopause Layer: distribution, times and the radiative influence of clouds" *by* A. Tzella and B. Legras

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We thank the referee for a thorough reading of the original manuscript as well as for the useful remarks he/she made. In what follows we address these comments and remarks (the referee's comments are cited in italics; unless stated otherwise, we will refer to the original manuscript for any changes made).

General comments

The paper shows, not surprisingly, that the biggest effect in the results comes from

using all-sky radiative heating rates vis a vis clear sky heating rates. However, there is not much attempt to analyze these heating rates, except for Figure 1. In this context, an important point about seasonal variation in the potential temperature of cloud encounters is essentially relegated to referring to a heating rate figure in another paper (page 18181). Given the importance of the heating rates, more information about them (e.g., seasonal variation) should be in the paper.

A new figure, which is the second one in the revised version, has been added. It shows monthly-averaged vertical profiles of heating rates for January and July 2005, calculated over several key regions (the same as in Figs. 3 and 6 of the original manuscript). These heating rates show a strong anomaly over the continental part of the Asian monsoon during summer. This anomaly is due to the slope of the LZH surfaces poleward of 20N as already indicated on p.18181.

The authors have examined the effect of changing the brightness temperatures of the clouds. Typical convective cloud tops are anvil-shaped, whereas the brightness temperature approach yields an umbrella shape, which means that a fair number of parcels see higher cloud top temperatures than are realistic. The 5K difference is only valid near the center, in the most optically dense portion of the cloud. Also, a cloud may influence a region around it. I wonder how the results would change if a "radius of influence" were included (i.e., the cloud were effectively larger). This would have a different effect than simply raising or lowering the clouds a little bit. I do not suggest redoing calculations here, merely doing some thinking and discussion of the issue in the paper.

The coldest brightness temperatures of a convective system are recorded close to its core which is shaped like a dome. The brightness temperature of the thick surrounding anvil is usually warmer due to differences in altitude as well as ice water content but the difference is usually not larger than 5K. Our application of a negative temperature shift uniformly is equivalent to rising of the whole cloud. We have shown that a shift by 5K in the brightness temperature only has a small effect.

We note that large convective systems influence their environment by inducing a divergent motion at the level of detrainment which attracts back-trajectories. Although this process is imperfectly represented within weather forecast models, it might be a reason for the relative insensitivity to the temperature offset. It should be recalled also that the pixel size of the CLAUS dataset is 30km (actually a sampled 5km $\times$ 5km pixel among a box of 6 $\times$ 6 boxes) and hence it is not possible to distinguish convective cores at such scales.

There is no discussion of the seasonal variation of TTL temperature, and how its relationship to seasonal variation of brightness temperature (if any) might affect the statistics. Is this because the vast majority of cloud encounters occur below the part of the TTL with significant seasonal temperature variation, or is it due to the fact that differences in radiative heating are more important (which I think is the point of the last paragraph on page 18181)?

We do not see clearly how this variation of TTL temperature can be disentangled from the simultaneous migration of convectively active region between winter and summer and the influence of transport. During winter, convective sources are localized over Indonesia and western Pacific and detrained parcels travel within the coldest part of the TTL. During summer, the main convective sources are located within the Asian monsoon region, which is the warmest region of the TTL, and are ventilated by the Asian monsoon anticyclone. These phenomena have important consequences for the dehydration of air entering the stratosphere and have been extensively studied (Fueglistaler et al., 2005, James et al., 2008, Fueglistaler et al., 2009a).

The paper gets bogged down in more detail than is necessary, making it hard to read. Shortening this work would substantially improve readability. Some candidates for elimination/shortening: (1) last paragraph of section 2.1 – it is confusing and, I believe, unnecessary; (2) Last two paragraphs of section 2.2 – see below; (3) discussion of figure 7 is opaque and very difficult to follow – perhaps eliminating it here and taking up the topic in a future short paper would be an appropriate course (I believe the problem

addressed is important); (4) eliminate Figure 9 and discussion.

The last paragraph of 2.1 has been removed, Table 1 has been removed, figures 9 and 10 have been modified and simplified to avoid redundancy and the text has been reworked and shortened in several places mentioned by the referee to improve clarity and remove unnecessary details.

Specific comments

Page 18168, line 5-6. *I don't understand this sentence. What does "this study" refer to?*

"Ploeger et al." has now replaced "this study".

Page 18170, lines 6-16. *I think that the last statement in this paragraph is defensible. I also think that the current weight of evidence does indicate that, for most constituents, convective injection above 380K is a relatively small effect. However, calling convective injection above 380K "spurious" is simply wrong (perhaps this is an English usage issue?). There are numerous examples from aircraft data indicating enhanced water from convection above the cold point tropopause (Kelly et al, JGR 1993; and, of course, Schiller's paper referred to in the text). Though stratospheric convective injection may not have a substantial impact on water in the global tropics, it clearly does have an impact in the North American summer monsoon region. It is correct that only a small percentage of the systems reach the tropopause, but these are often the biggest systems (as Liu and Zipser point out). Given the time scale of 15 days (30 days in the boreal summer) to reach 100mb (more to reach the Cold point tropopause, which is higher) via the Lagrangian mechanism, convective injection may be important for some short-lived compounds (methyl iodide has a lifetime of a week or so). By all means, use the algorithm presented to do the calculations, and defend it (as is done). But the authors are overstating their case.*

We have removed the word 'spurious' as it was unnecessary and ambiguous. It was not referring to the fact that clouds in the stratosphere are spurious but that the brightness temperature alone cannot distinguish clouds above and below the tropopause with the same temperature. We have chosen the conservative hypothesis to forbid any cloud above the cold point.

Page 18171, lines 3-23: *I find this discussion confusing, and perhaps unnecessary. I think that the authors are arguing that their method is better. However, the attempt to compare their results with Wright et al (Tibetan plateau, page 18175) does not refer to the method difference, so I am not sure what the point of including this is.*

A general comment on why we have not used full diabatic heating rates is now moved to 2.1. A short discussion of the results obtained with full heating rates is now included in 3.1.5 and 3.2.2. It is also mentioned that the discrepancy of Wright et al. with our results is less pronounced if we take into account that their definition of the Tibetan plateau includes the southern Himalaya slope which is indeed a very active region.

Page 18175, line 22: *Noticeable difference from what?*

A noticeable difference from results obtained from clear-sky conditions. The paragraph is now shortened so that any comparisons are clearly between the ALLSKY-DT0 and CLRSKY-DT0 ensembles.

Figure 4: *I think this point can be made without a figure. Day to day variability of convection is not a surprise. If it is, the authors should say more about it.*

Indeed the high temporal variation of convection can be deduced from sec. 3.1.2 where the spatio-temporal localization of the sources of convection is analyzed. But figure 4 also serves to compare the temporal variation of all-sky sources with those obtained under a clear sky. The clear-sky sources are shown to fluctuate more than the all-sky ones. The same point can, however, be deduced from Fig. 5 where it is shown that the most localized distribution is obtained under clear-sky conditions (an explanation is provided in the same section). We thus have now eliminated Fig. 4 and the discussion around it (l. 28ff, p. 18175).

Page 18181, line 1: *Could the authors speculate on the reason for this noted fact in the calculations?*

We may speculate that the lack of an obvious such relation is a combination of the effect of a transport barrier (imposed by the LZH) and the rapid decrease of deep clouds with altitude (as observed from BT data). Upon application of a bias on the BTs, the barrier effect dominates the subsequent shift in in the cloud top heights. This is, perhaps, the reason why the resulting shift in the vertical distribution of the sources is smaller than one would expect by solely looking at the shift in BTs.These points have now been added in sec. 3.1.5

Figures 9 and 10: *Perhaps only one of these figures, and resulting discussion, is necessary. I would eliminate Figure 9.*

We think that the difference between the first-entry time distributions and the transit-time distributions is an essential point that should not be discarded from the paper. We have, however, simplified Figs. 9 and 10 to avoid any redundancy and shortened their discussion.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18161, 2011.