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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

Anonymous Referee #3

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The goal of this paper is to examine how temporal and spatial fluctuations in convection, dynamics, and radiative heating rates affect the statistics of convective input into the stratosphere. The approach is to calculate a large ensemble of trajectories, and trace these through 3-dimensional (lon,lat,time) fields of cloud top altitudes from geostationary meteorological satellite data. All the dynamics and the radiative heating rates are calculated by model analyses (ECMWF Interim).

Overall, the results are publishable, subject to addressing the issues below. If the authors could do one thing, it would be to shorten the paper. Some suggestions on this

are included.

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.

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.

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)?

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.

Specific comments:

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

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.

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.

Page 18175, line 22: Noticeable difference from what?

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.

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

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

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