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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 #2

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The paper by A. Tzella and B. Legras presents a novel method combining Lagrangian modelling and observation archives of convective sources (from brightness temperature fields) to investigate transport of air masses detrained from convection in the TTL in terms of main detrainment locations and timescales to reach the upper TTL/Lower stratosphere. The effect of cloud heating is studied using both all-sky and clear-sky radiative heating rates to drive the vertical transport in the simulations. Geographical and seasonal features of convective sources and transport timescales are also quantified. It is clear now that no universal or exhaustive method exists to investigate TST and inner TTL transport features but it is always interesting to get results from different approaches, especially when the work focuses on the impact of convection and associated clouds on this transport. Through its originality this combined Lagrangian/observation technique brings nice contribution to the general scientific topic of Stratospheric-Tropospheric Exchange and the idea deserves to be published in ACP. The manuscript is well-written with a complete set of references, a big effort to describe of the used method and discuss the consistency of the results obtained. I however recommend the publication of this paper in ACP once the comments given below have been clarified.

Main comments:

- Data and method The authors necessarily consider and discuss various sources of uncertainties in their trajectory calculations. They are particularly aware of the uncertainty resulting from sub-grid cloud processes in ECMWF calculation of all-sky heating rates. The trajectory launch frequency (4 days here) is likely to be an important point which is investigated by the authors and eventually ruled out by sensitivity tests (p18168). Some aspects of the method may however deserve to be addressed or clarified. The first one deals with the period chosen which is of 1.5 year (from January 2005 to 30 June 2006). Why not having focused only on the year 2005 since the chosen period encompasses 2 winter seasons (JF), 2 springs (MAM), and only 1 summer (JJA) and 1 fall (SON) ? Indeed, one may think about a problem of too much weight attributed to the winter and spring events in your statistics. Also, the results section (section 3, p18173, l13) does not seem to take into account the half year of 2006 (like Fig.1 anyway). Could you explain this discrepancy?

Another point is the 200-day backtrajectory integration time which is a long and rather uncommon duration for trajectories used for TST studies. Such a long integration time might be problematic when mixing processes are discarded, a limitation of Lagrangian methods which is well-known anyway. Fueglistaler et al. (JGR, 2004) and Fueglistaler et al. (JGR, 2005) have used trajectory durations of 60 and 90 days (at maximum) re-

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spectively. Have the authors made some investigations using shorter integration times closer to other studies available in the literature to especially ensure that the mapping of trajectory-cloud intersections (Fig.3), the distributions of cloud top brightness temperatures (Fig.6) and potential temperature levels of trajectory-convection encounter (Fig.8) are conserved? If not, I would recommend this sensitivity test at least for one of these 3 figures.

- Latitude range The 50°N-50°S latitude range used to initialise the trajectories is wide and encompasses midlatitude transport features. I still do not get why the authors have not selected a narrower latitude range, I mean more restricted to tropical latitudes such as 30°N-30°S (which still encompass the influence of the Asian Monsoon). The paper focuses on TTL transport and it seems more interesting to analyze your nice statistics obtained from the tropical latitude range, especially concerning the remaining "free" trajectories likely to be significantly influenced by midlatitude to tropics transport (trajectories not influenced by tropical deep convection) when computed over a latitudinal area as wide as 50°N-50°S. This may allow the reader to better compare the convectively-influenced trajectories and the "free" trajectories with transport features mainly inherent to the tropics. Can the authors provide some of their statistics over this restricted latitude range to ensure that these new results compare well with the 50°N-50°S results? Also, I see that Fig.3 maps only the 30°N-30°S area which is in line with the above-mentioned point. This raises the following question. Has the transport distribution calculation method (Section 3.1.1) been really applied to the 50°N-50°S (there is a doubt from the choice of 30°N-30°S on Fig.3)? Fig.11 is plotted over 50°N-50°S and it would be worth to map the same latitude range for uniformity of Fig. 3 and 11.

- Pressure and potential temperature coordinates All over the manuscript the authors specify two levels of pressure (100 and 70 hPa) on which are released the backward trajectories while most of the convection/long-range transport are discussed in terms of potential temperatures. This is a bit surprising and misleading since most of the time trajectory studies either focus on pressure or pot.temp. coordinates mainly depending

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on the studied processes, i.e. diabatic or quasi-horizontal transport (see Hegglin et al., GRL, 2005 for instance). The simultaneous use of pressure and potential temperature values leads to some kind of ambiguity. Please explain this choice of initialization on pressure levels rather than typical potential temperature levels in your simulation and specify the range of potential temperature values corresponding to 100 hPa and 70 hPa.

- Trajectory wording The reader may be misled when the authors state about the initial and final positions of the backtrajectories. In some cases, this is clearly mentioned but not anywhere in the manuscript. For instance, when the authors write “trajectories end up” (p18172, l22) I guess this is not equivalent to the locations where backtrajectories are released but the final position of the trajectory backward in time. Also in Table 1 caption, “initial locations in the extratropics” refer to locations where the trajectories are released backward in time. So please clarify these points in the manuscript where wording referring to start/end or initial/final features of the 200-day backward trajectories may be doubtful.

Minor comments and technical points:

Fig.8 and Fig.11 and text: I think the term “histogram” is not appropriate. Distribution may be more convenient.

P18166, l23: “radiative heating rates” is written in italic.

P18169, l17-21: In this case, when a cloud top has been encountered backward in time, please clarify if the trajectory is stopped or still integrated up to 200 days to derive further convection source areas. It seems that (on the opposite to the above-mentioned Fueglistaler et al. papers), but not explicitly mentioned, you let all your backtrajectories run to the end of the 200-day period unless I have missed something important in your manuscript. . .

P18170, l29: “Less” instead of “least”.

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P18172, l9-10: This sentence is unclear to me. I do not get the same percentage numbers unless you also consider here parcels released at 70 hPa?

P18174, l19-21: Do these percentage values correspond to an all-area average for a given season? Please clarify.

P18175, l7: It is not clear whether or not these percentage values correspond to maximum values. Please specify.

P18175, l28: Fig.4 needs some clarification. Does it correspond to an all-area average? Why do the authors so low probability values? Is it because the normalisation method described in 3.1.1 is applied all over the 50°N-50°S latitude range ?

P18177, l11: "Less" instead of "least".

P18182, l4: Please specify "forward in time" in the caption of Table 5 and Fig.9 to avoid confusion (see trajectory wording main comment above).

P18182, l16-17: I am a bit confused here. Are you sure it is 370K and not 380K? I do not get the same time values.

P18183, l1: "…for clear-sky conditions". Refer to Table 5.

P18183, l6-8: This sentence is misleading. Do the authors mean all-sky LZH? Otherwise this statement is confusing (maybe except for the JJA season) because as mentioned in the introduction 345-355K are the main levels for maximum convection outflow and C-LZH is about 360K and A-LZH is about 355K.

P18184, l4: "2005" instead of "2004" I suppose.

P18185, l24, p18186, l1, l5: Are these percentage values obtained by geographical integration? Please clarify.

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