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Atmospheric
Chemistry
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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 and thoughtful reading of the original manuscript. 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).

Major comments

1. In many parts of the paper the readability could be improved. First, the wording

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concerning the back trajectories has to be clear and unambiguous. An example where this is not the case is the use of the terms 'initial' and 'final' in this context, with final at one point denoting the last (end) time, at another point the first (start) time of back trajectories (see specific comments below).

The term 'initial' refers always to the location of parcels at the start time of the back trajectory. We have found two instances where this term was used inconsistently and they have been corrected. The term 'final', which is admittedly confusing when combined with 'initial', is no longer used and we use the term 'destination' alone to denote the location of the parcels reached when the trajectories are considered in the forward direction.

Second, too much numbers (e.g., percentages) are presented in the text. The main message could be strengthened if only the important ones would appear in the text.

Some percentages have now been taken out from the text where they were not necessary, e.g. on pages 18179 and 181180 while section 3.1.1 has been rewritten and simplified to improve readability. We maintain, however, that some percentages are necessary to present quantitative results.

Third, I had the feeling that some percentages appearing in the text are not consistent with the figures (see my specific comments below).

All specific comments regarding percentages are now fixed.

2. An integration period of 200 days is a rather long time for a pure trajectory study, a simulation which neglects any kind of mixing process in the atmosphere. Of course, this is a general problem for any kind of pure trajectory analysis and nevertheless the results of the paper are valuable and worth publishing. However, in particular the estimate of the proportion of 'free' trajectories (trajectories not originating at convective sources during 200 days of backward integration) seems problematic to me. Table 1 shows that the proportion of those free trajectories which end in the extratropics

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is rather small (1–6% in the annual mean). I think there is a much larger fraction of trajectories which are transported across extra-tropical regions for many days during the backward integration period and nevertheless finally end in a tropical convective source. As these trajectories may encounter regions of strong flow deformations (subtropical jets) for long times, they are likely to be influenced by mixing and will represent extratropical stratospheric rather than tropical cloud air. Hence, I think that the fraction of in-mixed extratropical air is larger than the stated 1–6%. Moreover, these annual mean values are not reflecting the strong seasonality in extratropical–tropical exchange and are therefore misleading. Even a small percentage of extratropical (in-mixed) air has the potential to influence the mean tropical mixing ratio of a tracer, if the horizontal gradient in the species' mixing ratio is large enough. E.g., the impact of horizontal in-mixing from the extratropics on the annual cycle of ozone in the TTL was recently shown to be important (Konopka et al., 2009). The authors should, at least, thoroughly discuss these points.

In this comment the referee is initially concerned with the fraction of trajectories that, during their back trajectory, are transported across extra-tropical regions. In particular, the referee comments on the small fraction of 'free' trajectories that end up in the extra-tropics that he finds from Table 1 to be between 1-6%. However, there is no such fraction shown in Table 1. This may be the result of some confusion between the terms 'initial' and 'final' (see major comment 1 above). For this reason, we here report and discuss the locations of 'free' trajectories at (a) the launch surface and (b) at the end of their back trajectory.

As noted in Table 1, the proportion of trajectories that are 'free' is between 17-23% for trajectories launched at 100hPa and between 39-47% for trajectories launched at 70hPa. Of these trajectories, the majority is at launch located in the extratropical stratosphere (74-93% and 62-69% of the total number of free trajectories for trajectories launched at 100hPa and 70hPa, respectively). These percentages can be deduced from Table 1 by dividing the entries corresponding to 'free' & $|\phi| > 30$ with those corre-

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sponding to ‘free’ parcels. (Note that all percentages shown in this table are calculated relative to the total number of trajectories with the latter including both free and CS-TTL trajectories). Note that the vast majority (>80%) of ‘free’ trajectories that are, at launch, located in the extra-tropics ends up, at the end of their back trajectories, at higher parts of the stratosphere (θ >400K), entrained within the (reverse) Brewer-Dobson circulation (percentages not shown in Table 1).

The proportion of ‘free’ back trajectories that end up with theta<380K is between 14-20% for the all-sky case and 40% for the clear-sky case for trajectories launched at 100hPa (these percentages can be deduced from Table 1). Of these trajectories, 12-17% are located in the extra-tropics at the end of their back trajectory (percentages not shown in Table 1). These percentages indicate that a significant fraction of trajectories is transported across extra-tropical regions. Of course a larger proportion of parcels (both CS-TTL and free) is transported across extra-tropical regions during their back-trajectory. But we here do not report on these values.

The referee is next concerned with the impact of horizontal in-mixing from the extra-tropics. This mechanism was shown to be important for ozone. However, the corresponding study for species originating from convection (the main focus here) has not been done. We can therefore only speculate on the impact of this mechanism on their mixing ratios. Moreover, in this paper, our emphasis is less on mixing ratios and more on the sources of convection and times for transport from these sources to the upper TTL and lower stratosphere, including paths wandering between the tropics and the extra-tropics. The air in the extratropical lower stratosphere is itself a mixture of air which has been recently in the tropics and air which has stayed for a long time in the extratropics and the upper branch of Brewer-Dobson circulation. The characteristics of a finite volume air parcel depend on the history of mixing along all the trajectories of particles ending in this parcel and is beyond the scope of this study.

Note that in the revised manuscript Table 1 has been removed; all discussion of this table is now part of sec. 2.4.

Minor comments

General:

1. In my opinion, the most interesting results of the paper concern the distribution of convective sources and their degree of localisation, rather than the time-scales of transport. For the time-scales of upward transport there already exist a large number of estimates in the literature. Moreover, Ploeger et al. (2010) have recently shown that these time-scales depend very sensitively on the particular trajectory method (e.g., the choice of vertical velocities). I would therefore recommend to focus more on the convective source distributions as the main results, at least in abstract and conclusions.

Indeed, in the literature, there now exist a large number of estimates for times for transport within the TTL. Examples are found in Fueglistaler et al. (2004), where residence times are calculated, and in Ploeger et al. (2010), who also consider transit-times and their dependence on the vertical velocities. In all these previous studies, the times are calculated by fixing the levels of potential temperature between which parcels are travelling and do not involve the capacity of clouds to bring parcels from the surface to the upper TTL and lower stratosphere. In this paper, we focus on the times for transport between detrainment (as defined in sec. 2.2) and a particular surface in the upper TTL adding an important dimension that was, to our opinion, missing in previous studies. Since the back trajectories detrain at different potential temperatures (see e.g. Fig. 8), and that the detrainment properties vary rapidly with altitude, the times for transport that we here calculate are different to the ones calculated in previous studies. This point has already been noted at the end of sec. 2.1 (1st paragraph p. 18168).

It is worth noting that the time scales calculated in our study are also closely related to the times for transport between the boundary layer and the upper TTL/lower stratosphere. As also explained in sec. 2.2, this is because the parcels' motion between the boundary layer and the point of detrainment is expected to be characterised by a rapid, vertical upward transfer that lasts several minutes (or at most several hours in the case

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of a thick anvil). The last detrainment model thus provides a different way to calculate transport between the boundary layer and the upper TTL.

We agree that the distribution of sources and their localisation are important and both results are already substantially mentioned in the abstract (first half of second paragraph) as well as in the conclusions (third paragraph).

Note that comments on the choice of radiative heating rates for the vertical velocities are provided below.

2. I'm unsure about the restriction to radiative heating rates (see also my specific comment below). Of course, in cloud-free air the latent heat contribution should be negligible. However, the residual heating (the sum of latent heat exchange, turbulent and diffusive heating) in ERA-Interim can be positive in the deep tropics up to 380 K in the annual mean (see e.g., Fueglistaler et al., 2009b) and it is not possible to separate the residual heat component into the individual terms. Therefore, important (non latent) heating terms could be neglected by restricting to all-sky radiation only. The motivation to use both all-sky and clear-sky heating rates is, in my opinion, to derive an estimate for the uncertainty in vertical transport, due to radiative effects of clouds. Wouldn't it be even better to run a third set of trajectories using total heating rates to take also the uncertainty due to the residual heating terms into account? And why did you include the $\pm 5K$ *brightness temperature offset only for the all-sky trajectories?*

The choice of radiative heating rates is made on a physical basis. As also mentioned in sec. 2.1, as long as a trajectory remains in cloud-free air, the component of heating that is associated with the release of latent heat can be discarded while the contribution of heat transfer by turbulent motion and diffusion was previously found to be small (Fueglistaler et al. (2009b)). In particular, an inspection of the residual heating within the TTL reveals its association with precipitation underneath which suggests that it is mainly due to latent heat release. The residual is always very small in the cloud-free upper troposphere and lower stratosphere except in a few locations as discussed in

Fueglistaler et al. (2009b).

Wright et al. (2011) has considered the last detrainment model but used total diabatic heating rates to represent vertical motion. However, we believe that using total diabatic heating rates may lead to erroneous transport. In particular, since the total diabatic heating rate is positive between the boundary layer and the tropopause, the descending parcel motions associated with cloud-free regions between convective regions will not be represented. This is particularly true for air parcels wondering around the fluctuating A-LZH (see also last paragraph of sec. 2.2).

A set of results obtained using total diabatic heating rates and BT=0 (now briefly discussed at the end of secs. 3.1 and 3.2) suggest that indeed, when compared to ALLSKY-ΔT0 ensemble:

- parcels encounter a convective source at distinctly lower potential temperature values (mean is 350K versus 354K for ALLSKY-ΔT0) and higher BT values (mean is 212K)
- the sources are less localised (similar to ALLSKY-ΔT-5 ensemble for which all clouds are raised high) and
- vertical transport is faster (mean exit-time from region below 360K is 11 days; mean first entry to 370K surface is 23 days. Corresponding ALLSKY-ΔT0 values are 15 and 27 days, respectively).

Our motivation to use both all-sky and clear-sky heating rates is to quantify the radiative influence of clouds (and not necessarily the uncertainty in vertical transport). We have only considered offsets in the brightness temperature for the all-sky case which is the physical one. We believe that showing the trajectory ensembles obtained with offsets in brightness temperature for the clear-sky case would complicate the reading of the figures and the message we wish to transmit.

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3. Why did you consider the annual mean for the year 2005 for some results and the average over 2005 and half of 2006 for other results? I would prefer annual mean estimates for 2005 for all results.

In this paper we consider only those CS-TTL trajectories that are detrained during 2005. These CS-TTL trajectories constitute 65-66% of the CS-TTL ensemble. This ensemble is obtained for both all-sky and clear-sky conditions by launching trajectories during 2005 and the first half of 2006. The remaining CS-TTL trajectories have either detrained during 2004 or 2006 and are discarded.

We use those CS-TTL trajectories that have detrained during 2005 to determine the sources of convection for 2005. In order that all of these sources are equally sampled, we set the launch period to cover the period between the 1st of January 2005 and the 30th June 2006 (Note that the latter date should actually correspond to the 31st of Dec+200 days=19th July but is shorter by 19 days. This slip will only have a small effect on the tails of the transit-time histograms for some December sources. Otherwise the effect is negligible and not worth mentioning in the text).

The above information is now added in the first paragraph in sec. 2.1 (where the methodology for obtaining the trajectories is described) as well as at the beginning of sec. 3 (where the sources are obtained). We hope that this makes the methodology clearer.

Specific and technical:

P18163, L24: *What do you mean by 'apparent at small time-scales'? I think there are significant differences in the transit time distributions, e.g., between the summer and winter case in Fig. 11a.*

Indeed, in Fig. 10a, there are some differences (though not large) between winter and summer as well as between regions. For this reason the relevant text in the abstract has been modified into: "The distributions of vertical transport times are wide and skewed ... while some seasonal and regional transport characteristics are apparent for times up to 60 days."

P18166, L18: *This sentence needs clarification and rephrasing: It is true that the difference in dispersion between kinematic and diabatic trajectories is less prominent in ERA-Interim than e.g. in ERA-40, as shown by (Monge-Sanz et al., 2007; Liu et al., 2010). However, there are differences with the vertical dispersion higher for kinematic compared to diabatic trajectories causing significant differences in reconstructions of ozone in the TTL (compare, Ploeger et al., 2011).*

The following sentence has been introduced to account for this point: "Although this difference is less prominent in the EI dataset (Monge-Sanz et al. 2007,Liu et al. 2010), it may still be significant in reconstructing ozone (Konopka et al. 2009, Ploeger et al. 2011)."

P18167, L2ff: *See my general comment above, concerning the use of radiative heating rates.*

A response to the general comment is given in your General comment 2.

P18167, L7:*'... kinds of clouds ...'*

OK

P16167, L26:*I wouldn't use the term 'barrier for transport', as this barrier exists only if important terms in the heating budget are neglected (only radiation considered). As shown by Ploeger et al. (2010), including the residual heating term (latent heat release, turbulent and diffusive heating) yields positive vertical velocities (upward motion) throughout the mean tropical velocity profile.*

Indeed, the term 'barrier for transport' refers to a simplified framework that helps conceptualize the need for clouds to penetrate the LZH in order to transport air across the TTL. If air detrains below (above) the LZH there is a low (high) probability that it makes its way to the upper TTL. What we find interesting is to examine how, once detrained

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from clouds, air crosses this barrier and this is the subject of our study.

We emphasize that vertical out-of-cloud motion is controlled by radiative heating and that the ascending motion in the tropics is fast and concentrated in convective towers that occupy a very small area. The mean ascending motion obtained by averaging this fast, localized ascent and the slow descent in cloud free air does not reflect the physical processes. Note that Fueglistaler et al. (2009b) have previously shown that the contribution of turbulent and diffusive heating to the total diabatic heating is small in the upper troposphere and the lower stratosphere (see also discussion of minor comment above - General 2).

Note that, unlike other transport barriers in the atmosphere, this transport barrie does not exhibit any discontinuity in tracer concentrations. This is because it is continuously ventilated by convection which detrains air of the same composition above and below the barrier. Thus, this barrier does not separate the composition of air but instead separates the convective flux in two parts: the first part consists of air that eventually enters the stratosphere while the second part consists of the bulk convective flux which is just recycled within the troposphere.

P18168, L7ff:*Why is the motion of those trajectories, which remain around their launch level for some time, 'spurious' if this motion is consistent with the wind fields and we trust the reanalysis data? And how does this impact your results? The following paragraph needs clarification.*

The motion is obviously not spurious if we trust the analyzed winds and heating rates. The point we made there is about launch frequency not the wind but it is minor and distracting. In order to fulfil the requirement of other referees to eliminate any unnecessary discussion, we have decided to remove this paragraph.

P18168, L26: *The term 'CS-TTL' is unprecise, as you include also extratropical air (latitudinal boundaries ±50). A few more words could be helpful.*

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The choice of term ‘CS-TTL’ refers to the ensemble of trajectories originating from convective sources. The latter are located in the tropics, more precisely within the TTL. Air detrained from these sources has to cross the TTL to reach the lower stratosphere (for both tropical and extra-tropical destinations). It is for these reasons that we have chosen the term ‘CS-TTL’. In other words, the TTL might not be the destination but all trajectories selected within the CS-TTL are detrained within it and need to go across it.

P18171, L25:*‘... is a run ...’*

‘Is run’ is now replaced by ‘is performed’ to maintain a formal language.

P18171, L25: *I would say ‘... two sets of trajectories ...’, as the number of different simulations you consider is four (ALLSKY-ΔT 0, ...).*

We disagree. For each launch level, the number of simulations is two (one for all-sky and one for clear-sky conditions). For the simulation performed under all-sky conditions, two additional trajectory ensembles are obtained by considering two offsets to the brightness temperature values (+5K and -5K) that modify the cloud top heights. The resulting CS-TTL trajectory ensembles differ with the value of the offset. However, all three all-sky CS-TTL trajectory ensembles (ALLSKY-ΔT0, ALLSKY-ΔT5, ALLSKY-ΔT-5) are obtained using a single simulation for the trajectories.

P18172, L9ff:*I don’t see where the numbers 17-30% and 40% come from (see my major comment). Of course, the proportion of CS-TTL trajectories encountering a convective source within the last 90 days is non-negligible, as already the proportion encountering a source within the last 40 days was found to be $\geq$ 40% (previous sentence, Fig. 2) and is therefore non-negligible. In my opinion, the 90 days proportion should be larger than the 40 days proportion, as the last 40 days are included in the last 90 days. And even more so for 200 days, as this is the whole trajectory length. Therefore, the 200 day proportion should be the total CS-TTL proportion of 79-80% (ALLSKY).*

The english is now changed in this part of the text as previously it was wrong and confusing. In particular, the phrase: “The proportion of CS-TTL trajectories that have encountered a convective source within the last 90 days and 200 days ...” has now become “The proportion of CS-TTL trajectories that have previously encountered a convective source between 90 and 200 days ago ....” It is hopefully now clear that these proportions correspond to Fig. 2 and they necessarily need to be less than 50%.

P18172, L20:*‘... originates ...’*

We maintain that ‘originate’ is grammatically correct as majority means a multitude.

P18173, L2:*That tropical upwelling slows down around the LZH, causing longer trajectory transit times in that region, was already mentioned by (e.g., Fueglistaler et al., 2004; Ploeger et al., 2010).*

To our knowledge, the first to report the region near the C-LZH as being a ‘stagnation region’ is Sherwood and Dessler (2003). Indeed, the existence of this so-called ‘stagnation region’ has also been observed in several trajectory studies (e.g. Fueglistaler et al. (2004); Ploeger et al. (2010)).

These references are now added in sec. 3.2.1 where the histograms of first-entry times are shown and discussed. Sec. 2.4 is merely reporting the presence of a stagnation region, as evidenced from the percentages of free trajectories obtained under clear-sky conditions below 360K.

P18173, L9:*I think, ‘final destination’ here refers to the last time in forward time (of the backward trajectories) - what you termed ‘initial’ before (e.g., in the caption of Table 1.; there, ‘final’ refers to the last time in backward time). I’m slightly confused about the use of ‘final’, ‘initial’, ... also at other points (see my general major comment).*

This terminology has been fixed. ‘Initial’ refers only to the starting position of the back trajectory and ‘final’ is no longer used to avoid confusion. We denote as ‘destination’ the initial location when trajectories are considered in forward time.

P18174, L11ff:*I would rephrase the sentence to improve readability: ‘We resolve the* *source distribution ... in order to obtain meaningful statistics.'*

OK

P18174, L19ff: *What do these proportions refer to? Their sum is 27+27+22+23 = 99% for ALLSKY and even larger than 100% for CLRSKY.*

These proportions refer to the seasonal differences of detrainment. They represent the percentage of CS-TTL trajectories that have detrained during a particular season. Note that these percentages are now discussed in a separate paragraph that does not involve the source density that may have been one possible source of confusion.

Their sum in the all-sky case is 99% because the numbers are rounded to an integer. For the clear-sky case there has been a mistake that is now rectified: The percentage of CS-TTL trajectories detrained during boreal spring is by 2% lower than the corresponding percentage in the all-sky case.

P18175, L5: *I would define the geographical locations only once in the caption of Fig. 3, and just refer to it here.*

OK

P18176, L3: *Why is the daily variability larger for clear-sky compared to all-sky conditions? Is it so, because horizontal transport around the LZH is more important for clear-sky, due to the slower vertical transport in that region?*

As explained in sec. 3.1.2, p. 18177, l. 10ff, the C-LZH is, on average, higher than the A-LZH, leading to a smaller proportion of air at higher parts of the TTL. In particular, the higher C-LZH leads to a smaller number of sources able to transport air at higher parts of the TTL. Thus, the source distribution is more localized in clear-sky than all-sky conditions. This higher spatio-temporal localization implies higher daily fluctuations for the sources. Since Fig. 5 implies Fig. 4, it has now been removed as well as the discussion around it (following suggestion by Referee 3).

P18177, L22:*I don't see the 'lack of seasonal differences'. The fraction of CS-TTL parcels varies between 27 (DJF) and 22% (JJA), see Table 2, so seasonal differences are about 20%.*

Indeed there are seasonal differences, but they are not large, and this is now emphasized within the text in sec. 3.1.1. As a result this particular line is now removed from the text. The fact that the degree of source localization varies little with the season is, however, still interesting.

P18179, L20:*'... the deeper the convective sources, the less localised they are and the more efficiently they are sampled', to clarify what 'efficiently' here means.*

The sentence is now changed into: 'At the same time, the smaller the value of T* is, the deeper the clouds in are and thus they can be sampled more easily.'

P18179, L21ff: *'In particular, ...' – I don't understand the point here.*

This paragraph is now changed. Hopefully the last two points are now clarified. We decided that the word 'localization' in 3.1.4 could lead to confusion since we are referring to the partitioning of sources among clouds that reach a given level and not to the density of such clouds themselves and because it is used with this different meaning in 3.1.2. We replaced 'localization' by 'sparseness' in 3.1.4.

P18180, L16ff: *I wouldn't present all percentages in the text, to improve readability.*

OK

P18184, L24: *Did you check the influence of the monsoon, e.g. by checking that a large fraction of parcels circles around the anticyclone?*

No. The influence of the monsoon is left for future work.

P18187, L27: *'... advantages compared to ...'*

Both "has two main advantages over other Lagrangian models" and "has two main

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advantages compared to other Lagrangian models” are correct.

Table 4/caption: *‘... the focus is on the four ...’. I would end the caption at ‘... surface.’.*

OK

Figure 1: *I would write the indices (a) and (b) on top of the figures (better say ‘main convective outflow’ instead of ‘mean ...’).*

I have made the change of ‘main convective outflow’ but left the indices to be dealt with by the editorial office.

Figure 3: *Why are the clear-sky distributions more patchy? Is it just due to less CS-TTL trajectories causing worse statistics?*

No. the proportion of CS-TTL trajectories in clear- and all-sky conditions is not significantly different over 200 days (see e.g. Fig. 2). As also explained above (see our response in comment P18176, L3), the clear-sky distribution is more patchy because under these conditions, the C-LZH is higher and thus, fewer sources are able to transport air into the upper TTL.

Figure 5/caption: *‘... trajectories uniformly randomly distributed ...’*

We have now shorten this caption that was too long before and perhaps confusing.

Figure 8: *What is the bin size for the histograms?*

The histograms are calculated using a bin size of 2K in potential temperature (now added in the corresponding caption).

Figure 10/caption: *‘... also shown is the corresponding ...’*

OK

Figure 11/caption: *‘... summer (dashed) ...’, ‘... (in black)’, ’... different ranges of ...’*

OK

Figure 11: *I find it interesting that the peak of the histograms shifts from the deep tropics to the subtropics and extratropics with increasing transit time. I would mention this behaviour in the text.*

The transition between the different regimes is indeed already described in p. 18185, l. 18ff.

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

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