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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal and inter-annual variations in Troposphere-to-Stratosphere Transport from the Tropical Tropopause Layer" *by* J. G. Levine et al.

Anonymous Referee #3

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Report on "Seasonal and Inter-annual Variations in TST from the TTL" by Levine et al.

This is a well-written paper describing a well-organised trajectory study. ECMWF re-analysis (ERA-40) winds are used to investigate transport patterns within the tropical tropopause layer (TTL). The focus is on documenting the pathways for troposphere to stratosphere transport, taking the base of the TTL (at approximately 340K) as a starting point for forward trajectories. As with a previous work (Levine et al., JGR, 112, D04308, 2007), the approach differs somewhat from previous trajectory studies of the TTL because the motivating scientific problem is the transport of very short lived halo-

genated species (VSLS) into the stratosphere, as opposed to understanding changes in water vapour concentration in the stratospheric overworld. Restricting the study to timescales relevant to VSLS (4 weeks) allows several relevant new aspects of TST from the TTL to be identified. The main new results relate to
(i) the seasonal variation of transport into the subtropical and extratropical lowermost stratosphere,
(ii) the hemispheric distribution of transport into the lowermost stratosphere (transport to the southern hemisphere is significantly greater),
(iii) the interannual variability of TST to the lowermost stratosphere, evaluated by comparing years 2001 (baseline), 1998 (EL Nino) and 1999 (La Nina).

I would be happy to see this paper published in ACP after consideration of the comments below. My main concerns relate to some of the interpretation of results (notably the assumption that time-averaged wind fields are a useful diagnostic of the transporting circulation), and a surprising result that I would like to see investigated more thoroughly (transport to the Northern Hemisphere extratropical stratosphere being found to be equally strong in winter as in summer).

Main comments

1. **Interpretation of time-averaged wind fields** It is naive to use time-averaged Eulerian winds on pressure surfaces to discuss the Lagrangian circulation (Figure 9 and pg. 503). A well-established feature of atmospheric flows is that eddy fluxes of tracer can have a similar magnitude to advection by the mean flow, so a tracer or particle might experience a quite different time-mean transport velocity to the Eulerian average shown in Fig. 9. Many authors minimise the eddy transport effect by using time-averaged heating rates as their measure of vertical velocity. The heating rates are a measure of the vertical motion relative to a (near-material) isentropic surface, and eddy flux effects are therefore largely

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filtered out. I suggest that either Fig. 9 and the accompanying discussion is removed, or that it is replaced by calculated heating rates, if available (although in the TTL analysed heating rates should be interpreted with care), together with v calculated on an isentropic surface. This is not to say that the conclusions drawn from Fig. 9 are necessarily wrong, just that they are ill-founded.

A further problem is the lack of recognition that, due to horizontal transport, most particles will not remain in the strong ascent region (e.g. western Pacific) for long enough to experience transport all the way from the TTL to the overworld. It should be made explicit that four weeks is long enough for trajectories to meander horizontally throughout the TTL experiencing regions of both strong and weak ascent before experiencing TST. This is not clear in the present discussion surrounding Fig. 9.

2. **TST in Northern winter versus Northern summer** It is very surprising to find more transport into the Northern extratropical lowermost stratosphere in winter rather than summer. As discussed in the paper, this result seems to run counter to the measurements reported in Randel et al. (2001), Prados et al. (2003), and Hoor et al. (2005), as well as numerous modelling studies from Chen (1995) onwards (see also e.g. Haynes and Shuckburgh, 2000). The discussion on pg. 502 suggests some explanations for the discrepancy (e.g. neglect of TST originating outside the TTL) but is not (to me) entirely convincing. In particular the result seems directly to contradict that presented in Fig. 3 of Berthet et al. (2007). There, many fewer back trajectories are shown to originate in the tropics in winter as opposed to summer, throughout the depth of the lowermost stratosphere (albeit for January 2002 versus July 2002, although Fig. 5 suggests the result is robust over a number of years). One possibility that might account for the result is that a high proportion of the particles measured as having undergone TST are in fact in the 'tropopause region' and have not therefore undergone true TST. Previous trajectory studies of stratosphere to troposphere transport have usually

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taken care to eliminate trajectories undergoing 'shallow TST' (e.g. Wernli and Bourqui, 2002; James et al. 2003) by for example by employing a residence time criterion. I would like to see some sensitivity tests of this type applied to the current study, to test the robustness of the Northern winter / Northern summer result in particular.

Other comments and suggestions

1. Pg. 491. line 14. 'This homogeneity contrasts with the findings of recent studies...'. I don't believe that a fair comparison is being made here... the present result (of homogeneous source regions) is for transport to anywhere within the stratosphere (overworld + lowermost stratosphere), whereas the quoted studies are for transport to the overworld only. In fact the results reported in 3.6 for transport to the overworld only seem to support the previous studies. This sentence should be replaced by a more nuanced one.

2. Pg. 494. line 17. A comment might be added here noting that a further disadvantage of using the cold point as a tropopause definition is that it is not 'fluid following' in any sense, and may consequently jump around in space, with attendant disadvantages for calculating transport. The same disadvantage applies to the WMO tropopause definition.

3. Pg. 495 l. 9. There is little comment on the subjective nature of the timescale chosen here (4 weeks) or the extent of sensitivity to tropopause definition (other than the rather large change between dynamical and WMO definitions). Have such sensitivity tests been carried out? It would be good to emphasise the degree of robustness of the results here.

4. Pg. 492 and Figure 2. I think that Figure 2 could easily be improved by splitting the 'subtropics' and 'extratropics' into NH and SH components. There would then

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be no need for Fig. 4. The flow of the paper would also be more natural if section 3.4 discussing the NH/SH split of TST followed section 3.2. A further advantage would be that the new Figure 2 would then clearly illustrate the NH/SH split of TST into the lowermost stratosphere, as opposed to the NH/SH split of all TST. The former quantity seems to me to be more fundamental (as it seems likely that the eventual fate of air transported into the overworld is similar regardless of whether it enters just north or south of the equator).

5. Pg. 497 l. 8. It might be helpful to list all the longitude bands with the corresponding name (either here or in the Figure caption) to save the reader from having to work them out.

6. Pg. 503 l. 18 'The seasonality in vertical velocities is dominated by...'. The meaning of this sentence is not very clear... I presume that what is meant is that the change in position of the region of maximum ascent is responsible for most of the seasonal variability in vertical velocity...?

7. Pg. 505. l. 18. Although the presence of a PV gradient usually implies a mixing barrier, it doesn't necessarily follow that stronger PV gradients imply stronger mixing barriers. The barrier strength would also depend, among other things, on the level of eddy activity in the barrier region. Care should be taken to phrase this paragraph so as not to suggest this... the comment at the end about high resolution modelling being necessary should probably be emphasised more.

8. The authors should be commended for the accuracy of their proof-reading!

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