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Comment

# ***Interactive comment on “A global climatology of stratosphere-troposphere exchange using the ERA-interim dataset from 1979 to 2011” by B. Skerlak et al.***

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We thank the referee for his/her detailed comments, which were helpful to improve the presentation of our results. In addition to several smaller changes, a sensitivity study has been added to quantify the influence of the choice of the trajectory starting grid (showing that the original setup was appropriate) and of the choice of the PV-threshold used to define the dynamical tropopause. We now also discuss the relationship between different PV-isosurfaces and the WMO tropopause to some extent. The method is described in more detail and the units of all monthly averaged ozone fluxes have been changed to [Tg/month].

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*This study uses an established Lagrangian trajectory method with ERA-Interim re-analysis fields to produce a climatology of mass and ozone STE, including “deep exchanges”. This work is similar to, but expands upon and modifies, an earlier study that used the shorter timespan ERA-15 reanalysis. Appropriate comparisons of both similarities and differences with this and other earlier studies are made. Overall, the presentation is well-structured and well-written. Many of the results presented should be of interest to the readers of ACP. However, I have several concerns that need to be addressed before I can recommend publication.*

### Primary Concerns

*1. Many pathways of STE involve considerable stretching and/or mixing of air parcels. The parcel sizes assumed for the trajectories in this study seem rather large and likely to be subject to this stretching and mixing which would impact the results, particularly the magnitudes of exchanges that are quoted. Has there been any sensitivity analysis done with respect to the horizontal and vertical resolution of the trajectory parcels? How did the authors decide to use this resolution grid? The sensitivity of the results to the grid spacing needs to be quantified. I would expect that the net transport would be a strong function of grid size until the grid scale became less than the spatial scales of the STE processes.*

We now mention the limitations regarding stretching/mixing in the discussion. The grid spacing has been chosen to roughly match the spatial resolution of the ERA-Interim data (we interpolate the T255 data onto a 1x1 degree grid). In the new Sect. 5.2., we perform a sensitivity analysis using a finer trajectory starting grid (halved in both horizontal and vertical dimensions, leading to an 8-fold increase in the number of trajectories) and find that the results are only very slightly changed when using the higher resolution. Thus the trajectory spacing chosen is appropriate for ERA-Interim data. Note in general, that the critical resolution issue is related to the resolution of the underlying wind fields. Clearly, much higher-resolution data than ERA-Interim would better capture, e.g., orographic flow distortions and the mesoscale details of upper-level fronts,

but currently no such global dataset exists over climatological time periods. The setup of our Lagrangian diagnostic must be compatible with the resolution of the underlying meteorological data, but it cannot overcome important limitations in the representation of the STE processes themselves. The additional sensitivity test clearly shows that the original setup leads to robust results, given the resolution of ERA-Interim.

*2. Why is the ozone concentration of a parcel considered constant after crossing the tropopause? The crux of the trajectory argument is that the air mass at the trajectory end point is the same air mass at a point earlier in time (for forward trajectories). You have the 3-D ozone field from ERA. Why not look at what the ozone is at the end point? This concern doesn't really affect the annual STE estimates made in the paper but has great impact on the "deep STT" results.*

Thank you for mentioning this concern. In the original version we did not well explain our choice. A main reason for this choice is the better quality of the ERA-Interim ozone field in the stratosphere compared to the troposphere. We now state this in the methods section including a reference to Dragani et al. (2011). The comparison of our ozone concentrations at the tropopause to the measurements by Thouret et al. (2006, cited in the manuscript) gives us some confidence in our method and the ERA-Interim ozone field at this height. Because most tropospheric chemistry is neglected in ERA-Interim and the assimilation of ozone data performs best between 100 hPa and 10 hPa, we chose not to use the ozone concentrations in the troposphere for our study. By using the tropopause values of ozone, we obtain an upper estimate for the deep STT ozone flux, which is now mentioned in the paper. Nevertheless, we compared the results of deep STT ozone fluxes using the two different methods (ozone at tropopause and ozone at PBL top). We found that while ozone concentrations at the PBL top are lower than at the tropopause (as expected), there is no seasonal cycle or strong geographical pattern in this difference field. Thus we prefer to use the 'more trustworthy' ozone values at the tropopause and interpret the deep STT ozone flux as an upper boundary on the stratospheric influence on near-surface ozone.

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*The values of deep STT shown in Figure 15 and quoted in the text are somewhat misleading. I would assume that the values are upper bounds to the amount of ozone transported to the boundary layer (assuming 100*

All fluxes in our study are sensitive to a range of parameters as described in the new Sect. 5. As Hall and Holzer (2003, cited in the manuscript) have shown, the flux across a control surface such as the tropopause is never a unique, well-defined value but depends for example on the minimum residence time (explicitly demanded in Lagrangian methods but implicitly also present in Eulerian studies, see Bourqui (2006, cited in the manuscript)). It is thus in our opinion clear that all the results have to be interpreted with the limitations in mind.

*3. Related to the last point, it is not surprising the ozone flux follows the power law (Page 11558 and Figure 18) since the ozone concentration is held constant for the parcels after crossing the tropopause. The ozone flux in this case will behave just like the mass flux. I don't see the significance of this discussion in the paper considering the use of this assumption.*

Both the mass and the ozone flux follow a power law. We did not intend to state it as an important result that this is true for the ozone flux (compared to the mass flux) but to illustrate that the flux across the tropopause (mass and ozone) follows a power law even for fairly large minimum residence times. Hall and Holzer (2003) found that this is true for very small minimum residence times and we show that values of  $\tau$  up to 96 h have to be considered 'small' in the context of STE. To reduce confusion, we now present the results for the mass flux (Fig. 21) and summarize the results for the other fluxes in the supplementary material.

*4. I share the concern listed in the review by C. Homeyer regarding the choice of PV surface used to define the tropopause. I would add to his discussion that Schoeberl (2004; cited in the manuscript) shows that a 2 PVU surface tends to be more than 1.5 km lower on average than the lapse rate tropopause in a UKMO assimilation.*

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We now show the zonal average of the 2 pvu, 3.5 pvu, and WMO tropopause from which it is also visible that the 2 pvu surface is typically situated below the WMO tropopause (Fig. 15).

*Other studies, such as several by L. Pan using tropopause coordinates, clearly show that such an offset would have a great impact on the diagnosed constituent flux using Lagrangian methods. The mass flux is also likely greatly affected given the sharp change in static stability, etc. at the tropopause. Indeed, the authors show that the magnitudes of STT and TST of mass are greatly reduced using a 3.5 PVU definition (page 11562). I wonder if the seasonality is also changed. Also, the impact on ozone STE is not discussed (which is not simple to deduce since the mass flux is less but the ozone concentrations are most likely greater at 3.5 PVU).*

These aspects are now discussed in some detail in the new Sect. 5.1.

*I understand that the PV surface and lapse rate tropopause relationship may be different between analysis systems and with advances in analysis systems. I believe it would be helpful in this paper to examine the relationship between the PVU surfaces and the lapse rate tropopause in the reanalysis similar to that done by Schoeberl. This will help provide context for the results and facilitate comparisons to past and future studies.*

See answer above (we now show the 2 pvu, 3.5 pvu and WMO tropopause in the zonal mean in ERA-Interim in Fig. 15). However, we would like to emphasize that the most appropriate definition of the tropopause is a long-standing question. The WMO tropopause is often used as a reference, but it has its strong weaknesses. For instance, it exhibits strong discontinuous jumps in space and time, which is particularly critical when considering STE. We therefore do not think that the PV-surface matching best with the WMO tropopause is necessarily the most appropriate one. The best solution would be to quantify the flux across a stack of surfaces (between, e.g., 1.5 and 10 pvu), but given the very high computational cost of our approach, this has so far been

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technically impossible.

### Minor Issues

*Page 11539, Lines 6-11: These statements are contradictory. “most ozone in the troposphere is produced photochemically” vs. “stratospheric contribution to ozone in the troposphere could be as large as that from net photochemical production”.*

We do not fully agree with the statements being contradictory since it is only the net production (production – destruction) that is roughly equal to the stratospheric contribution. The gross photochemical production is much larger. But we have re-written the first few paragraphs of the introduction to avoid this confusion.

*Page 11541: What is the time step of the trajectory integrations?*

The time step for trajectory calculations in LAGRANTO is 30 min when using 6-hourly data. We obtain the wind fields by linearly interpolating from the 6-hourly data (ERA-Interim) and integrate the vector field using an iterative Euler scheme (3 iterations).

*Page 11541: I am curious as to how many trajectories re-cross the tropopause after the 48 h minimum residence criterion is met.*

Eventually, all of the trajectories will re-cross the tropopause. Within the 9 days analysed in this study, the information about re-crossing can indirectly be obtained from the dependence of the ozone or mass flux on the minimum residence time. For example, trajectories re-crossing 56h after the exchange will contribute to the mass flux using  $\tau=48\text{h}$  but not to the one using  $\tau=60\text{h}$ . Somewhat surprisingly, the dependence of deep STE fluxes is very similar to the one for fluxes across the 500 hPa surface.

*Page 11543, Line 12: Please provide at least a very brief explanation of the basis used in calculating the PBL height.*

A brief explanation has been added in Sect. 2.3: “The PBL height in the ECMWF model is determined as the height at which the bulk Richardson number reaches the

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critical value 0.25, following Troen and Mahrt (1986).”

*Page 11543, Line 15: Don't the trajectory parcels originating in the tropics represent less mass since the vertical spacing is less (page 11541)? That's a large fraction of the total trajectories. (I assume the trajectories of lesser mass are correctly accounted for when summing the mass flux!)*

You are correct and this is correctly handled in our method. This is now more clearly stated in Sect. 2.1: “ In the tropics (between 30 S and 30 N) the vertical grid spacing is 10 hPa to accommodate for the typically slower vertical motion.” “ Note that trajectories started in the tropics have a smaller  $p$  and thus a smaller  $m$  .”

*Page 11543, Lines 17-19: Globally the difference between the TST and SST should be 0 unless there is a trend in tropopause height. Am I correct in assuming that you present this as a check of the results? More should be explicitly said about why you discuss this.*

Yes, we looked at the global net flux in order to check the results. The finding of a positive net downward flux is in agreement with studies showing an increase in tropopause height and we deemed this worthy of a brief discussion in Sect. 3.5.

*Page 11544, Lines 15-16 and 25-26: Describe how this is an explanation for the local-ization. In particular, a low tropopause does not necessarily mean there is STE.*

This is correct and we have removed these misleading sentences. For STT, the peak in JJA might be due to increased frequency of deep tropopause folds (Sprenger et al., 2003). Because tropopause folds are very rare in this region in DJF, they cannot alone explain the peaks in TST mass flux that are present during the whole year. This region deserves more attention in further studies.

*Figure 5: These map figures would benefit from being a bit bigger. I realize this could be from the document creation by ACPD but it is hard to see some discussed features such as described on page 11547, line 2.*

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We have tried zooming into the latitude range [50S,70N] but the visibility of the mentioned feature was not much better. In fact, it is a weak signal and we do not discuss it in the revised version of the manuscript.

*Page 11549, Lines 3 and 11: I don't really see two "peaks" in each of these cases.*

The peaks are now renamed as 'secondary maxima', which is technically only true for the seasonal averages, where they truly form a local maximum. We decided not to show all the seasonal averages in the manuscript.

*Page 11554, Around Line 17: Why do you need to do all the averaging and approximating? You have the reanalysis, so you have the tropopause pressure, surface pressure, etc. at every grid point.*

We have now calculated the mass of the troposphere for every 6-hourly timestep and reach quite similar magnitudes for the trend. The paragraph has been rewritten: "To further analyse this, we have calculated the area-weighted average pressure at the tropopause and the mass of the troposphere for every 6-hourly timestep of ERA-Interim from 1979 to 2011. Linear regression analysis reveals..."

*Page 11555: More needs to be described about the fields used to determine the ozone flux. Is daily output used? Is it instantaneous or time-averaged? Is it temporally interpolated to the trajectory tropopause crossing time? The tropopause height and ozone at a grid point can rapidly and significantly change in events with significant crosstropopause transport (e.g., Price and Vaughan, 1993, QJRMS; Lamarque and Hess, 1994, JAS; Olsen and Stanford, 2001, JGR). Information on the ozone fields used will provide context of the uncertainty of the flux estimation.*

We now explain the procedure for the calculation of the ozone flux more clearly in the methods section: "The STT ozone flux is calculated analogously with  $\Delta m_{o_3} \approx M_{o_3}/M_d \cdot \Delta m \cdot [O_3]$  where  $M_{o_3}$  and  $M_d$  represent the molecular weights of ozone and dry air, respectively. The ozone concentration at the crossing,  $[O_3]$ , is obtained from

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linear spatial and temporal interpolation to the trajectory location.”

*Figure 16: Tg/yr seems like an odd choice of units for what appears to be monthly time series. Also, in the caption, do you mean “: : quantiles of the monthly values” instead of “annual values”?*

We agree and have changed the units for the ozone flux to Tg/month throughout the manuscript. To compensate for different lengths of the months, we have defined a ‘standard month’ by  $365.25 \text{ days} / 12 = 30.4375 \text{ days}$ . For example, the flux in January would be multiplied by  $30.4375/31$ . The captions in Figs. 16-19 have been changed.

*Page 11560, Last Paragraph; Can you relate this discussion more directly to the results of this paper? Can you estimate the uncertainty of your results due to convection using this information?*

We have added a sentence about the study of Gray (2003) where cross-tropopause transport due to sub-grid scale processes (convection) was estimated to be 38% of the total transport. Unfortunately, we don’t see how we can estimate this figure for our study given the intrinsic limitations of our method.

*Page 11561, Lines 5-6: “: : increased overall quality” of what specifically relative to this study? The trajectories?*

The increased resolution leads to more accurate trajectories and the improved and more extensive data assimilation removes artefacts in the tropopause structure present in ERA-15, especially in data-sparse regions.

*Page 11564, Lines 2-3: As I understand that study, the subdivision of ozone by Olsen et al. (2004) does not really allow for the calculation of TST fluxes of ozone.*

You are correct. The subdivision only allows for separate calculations of net flux of tropospheric and stratospheric ozone, respectively. In order to compare our results to Olsen et al. (2004), we calculated the net(STT-TST) ozone fluxes (see Fig. S10). Due to neglected tropospheric chemistry in ERA-Interim, the TST ozone flux is problematic.

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This is now discussed in detail in Sect. 6.3.

*Page 11570, (should be 11569) Lines 5-7: This point was made by Olsen et al. (2004). (And I see that he reiterates the point in a 2013 JGR paper referring to the ozone transport across the 380 K surface.)*

We have removed the second part of the sentence. “The ozone flux across the tropopause is significantly influenced by the seasonal cycle and geographical distribution of ozone concentrations at the tropopause.”

*Technical Correction Page 11566, Line 28: “and” should be “an”*

Corrected.

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*Interactive comment on Atmos. Chem. Phys. Discuss., 13, 11537, 2013.*

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