

Interactive
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.

Anonymous Referee #1

Received and published: 26 June 2013

This study uses an established Lagrangian trajectory method with ERA-Interim reanalysis 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

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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.

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. 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% accurate trajectories). Again, the parcels are subject to stretching and mixing, and in this case, tropospheric chemistry. I think it could be said that these PBL areas may be influenced by stratospheric air but the magnitudes should not be treated as a concrete result.

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.

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

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(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. 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). 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.

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”.

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

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

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

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

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the total trajectories. (I assume the trajectories of lesser mass are correctly accounted for when summing the mass flux!)

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.

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

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.

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

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.

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 cross-tropopause 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.

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"?

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?

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

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.

Page 11570, 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.)

Technical Correction

Page 11566, Line 28: "and" should be "an"

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

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