

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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Received and published: 21 September 2013

We thank Meiyun Lin for 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 PV-threshold used to define the dynamical tropopause, and we now discuss in more detail the limitations of our deep STT ozone flux estimates. Inspired by her comments, we have computed local trends in STT and TST mass fluxes from 1979-2011 at every grid point around the globe.

C7221

This review is conducted by Meiyun Lin at Princeton University. This study compiles a global climatology of STE for 1979-2011 using the ERA-interim reanalysis and a Lagrangian model. The analysis is relatively complete, including seasonal and regional variations, and some analysis of inter-annual variability for global integrated flux. I found the discussion of deep STT mass and ozone flux particularly interesting as it has implications for surface ozone air quality. Advancing knowledge on STE is crucial for understanding tropospheric ozone variability. This paper is certainly within the scope of ACP. However, the analysis has several limitations and uncertainties.

First, the authors quantify the ozone flux as a product of the mass flux and tropopause ozone from the ECMWF reanalysis, which is known to have large ozone biases as the authors also pointed out in the manuscript. But there are no thorough evaluations and analysis in the manuscript regarding how these biases in ECMWF ozone affect the estimated ozone flux.

In order to perform such an analysis, we would need to have access to an evaluation of the biases at the dynamical 2 pvu / 380 K tropopause. Unfortunately, we do not see how we could quantify the impact of the ozone uncertainties and biases on our estimates of the STT ozone flux. The best we can do is to mention in the paper (as done already in the previous version) that our ozone flux estimates of course directly suffer from inaccuracies of the ERA-Interim ozone field.

Second, the deep STT mass flux is done based on the ECMWF PBL depth, which tends to be biased high in winter (Siedel et al., 2012). You need to discuss how the seasonal biases in PBL depth affect the seasonality of deep STT and ozone flux, in particular winter versus spring maximum.

To our knowledge, Seidel et al., (2012) only mentioned the PBL height provided in ERA-Interim forecasts in one sentence in their section 2 (paragraph [7], last sentence): “Without showing a comprehensive comparison, we note that the ERA-interim product (i.e., with the ECMWF algorithm) shows higher heights, especially over high elevation

C7222

regions, than the algorithm used in this study. Differences are <100 m at night and several 100 m during daytime, and they are larger over ocean than over land.”

For the rest of the study, they applied the same algorithm to the ERA-Interim data as to the others to ensure consistency. Thus we can only base our answer on the quoted sentence above. Since most of the deep STT flux in our study occurs over land, the last part (larger over ocean than over land) seems not to be a problem. Also, this statement somewhat contradicts the statement about higher elevations. It would be interesting to see their exact results, but a bias on the order of a few 100 m should not alter our results significantly in most regions. The sensitivity to the PBL height over the Tibetan Plateau is now discussed in Sect. 6.1.

Third, the estimates of deep STT ozone flux in this study do not consider chemical and depositional losses in the troposphere. So the results may not be meaningful.

We agree that our approach cannot consider the effect of deposition and chemistry. However it is very detailed in terms of diagnosing the tropopause crossings and the pathways to the lower troposphere. The estimated deep STT ozone fluxes can only be seen as an upper limit on the stratospheric influence on ozone within the PBL. Clearly, these results have to be interpreted with the limitations of our method in mind.

Finally, the analysis of inter-annual variability for global mean flux may not be meaningful given the large regional variability of STE.

In addition to the globally integrated STE mass flux, we now analyse regional trends by compiling a time series of monthly averaged STT and TST mass fluxes at every grid point around the globe from 1979-2011. These time series allow us to identify regions with significant trends (see Sect. 3.5 and Fig. 13).

Specific comments:

Page 11539, Lines 20-25: some papers cited here do not focus on the impacts of STT on surface ozone air quality. You also need to cite Langford et al (2009) and Lin et al

C7223

(2012) here and some other places in the manuscript where you discuss the impacts of STE on surface ozone.

We have added these citations in various places throughout the manuscript.

Figure 4: Need to indicate in the caption that this is annual mean.

The caption has been changed.

Figure 7: I did not follow what this figure means exactly. Is the analysis meaningful? Do the gray lines indicate that the net (STT-TST) fluxes are always zero?

The figure shows zonally integrated cross-tropopause fluxes. The gray line in the left panel indicates zero net flux and helps to identify regions where the net flux is directed up- or downward. From the panels in the middle and on the right side, it is visible that deep STE only occurs in the extratropics and the air is moving equatorward (poleward) as it is descending (ascending) into the PBL.

Figure 12: This plot does not provide any insight into the temporal evolution of STE in the past 33 years.

We do not understand this comment. Why should a plot of a time series of STE over the past 33 years “not provide any insight into the temporal evolution of STE in the past 33 years”?

Is the global annual mean analysis meaningful given the large regional and seasonal variability of STT? Is it possible to do seasonally averaged time series of the STT and deep STT fluxes for specific regions, such as western North America, which is prone to deep intrusions in spring (as shown nicely in your Figure 5, also in Lin et al., 2012)? Then you can discuss inter-annual variability and the climate drivers, such as ENSO and NAO. I think such information should be very useful for understanding regional impacts of deep STT events on surface ozone air quality.

We are grateful for your clear statements that regionally integrated time series are also

C7224

relevant and of interest to the community. We thus hope that the regional trend analysis mentioned above and discussed in Sect. 3.5 is of great interest to you. A discussion of ENSO and NAO is, in our opinion, not within the scope of this paper.

Figure 13: You need to conduct evaluation of ECMWF ozone at the tropopause using ozonesonde measurements. One possible way is to superimpose sonde data provided in Logan et al., 1999 and Prather et al., 2011 in this plot. Then you can tie the agreements and bias to discussion of STT ozone flux in Figure 14. The high STT ozone flux in the central U.S. and Siberia in JJA (Figure 14) looks unexpected to me. Is this previously established? Does this have something to do with the biases of tropopause ozone from the ECMWF analysis?

We agree that it is necessary to validate the ECMWF ozone field against independent observations. This has to some extent been done by Dragani et al. (2011) but unfortunately not explicitly at the 2pvu/380K tropopause.

A possibility to compare our ozone values at the tropopause (shown in Fig. 14) to ozonesonde data from, e.g., Logan (1999) would be to first identify the average pressure (monthly) at the tropopause (2pvu/380K) at the location of the stations investigated and then read the monthly average at this height from the plot shown in the cited paper. Of course, we could also contact Ms Logan and ask her if we could get access to the data to make the comparison more accurate. It is in our view questionable if a comprehensive comparison (only possible for the time period 1980-1993) of ERA-Interim to this data set should be contained in the study presented here given the many aspects that could be discussed. We think it would be more valuable if such a comparison were performed as a future study.

A comparison to the study of Parther et al. (2011) is in our opinion not directly possible due to the different tropopause definitions used (e90 and lapse-rate vs. 2pvu/380K). A possibility would be to interpolate the ozone field in ERA-Interim to the lapse-rate tropopause at the radiosonde locations and compare them to the data provided in

C7225

Prather et al. (2011).

We have compared the seasonal cycle of ozone in ERA-Interim at the 2pvu/380K tropopause with in-situ aircraft measurements from Thouret et al. (2006) and the agreement is quite good, as discussed in Section 6.3 of our manuscript. Albeit this comparison is only valid for the North Atlantic, this is a clear indication for a reasonable ozone field at the tropopause in a hot-spot region of STE.

Figure 17: Is the abrupt shift near 2005 an artifact of the ECMWF reanalysis? Does it exist also in the mass flux or just the problem of the ECMWF ozone field at the tropopause? If this is the problem of the ozone field at the tropopause, does it make more sense to focus all ozone flux analysis on the period 1979-2004?

Yes it appears to us that this shift near 2005 indeed is an artifact due to changing satellite instruments, as discussed in Section 6.1 of our manuscript. We do not have the means to investigate the exact reasons for this jump in more detail. A focussing of the analysis on the period 1979-2004 would certainly be feasible, however it is not totally clear that the ozone data assimilation problems in ERA-Interim affect only the time period after 2005. All we know is that there is a discontinuity near this time. As can be seen from Fig. 19, the mean value of the STT ozone flux (global, black horizontal line) is quite close to a mean value of the period 1979-2004 (by visual inspection, we would say it is approx. 10 Tg/yr lower). The main effect of those spurious years on the results shown in the manuscript is the large inter-annual variability in certain months visible in Fig. 18(a-d). We have discussed this issue in Sect. 6.3 (last paragraph).

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C7226

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

C7227