Atmos. Chem. Phys. Discuss., 13, C4304–C4307, 2013
www.atmos-chem-phys-discuss.net/13/C4304/2013/

Atmospheric Chemistry and Physics Discussions

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: 28 June 2013

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

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

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

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?

Figure 12: This plot does 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.

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?

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?

References:

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