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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.

Anonymous Referee #2

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The article "A global climatology of stratosphere-troposphere exchange using the ERA-interim dataset from 1979 to 2011" by Skerlak et al. examines the problem of the flux of mass and ozone through the tropopause revisiting and improving a technique developed previously. The authors calculate an STE climatology for both mass and ozone. They examine the geographical distribution of fluxes as well as the seasonal and inter annual variabilities.

The paper is well structured and the results are relevant for ACP. However, the analysis is too much focussed on the 2 PVU∼380 K surface alone. It is true that a reference to the 3.5 PVU surface is present in section 5.2 and section 3.4 is devoted to an analysis of

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fluxes across different potential temperature surfaces. But overall the crucial sensitivity of the flux numerical value to the chosen surface is only discussed in the end as a minor issue. The definition of tropopause determines the numerical value of the fluxes. What would be the result using the WMO tropopause or other alternative definition (e.g. based on PV gradients – Kunz et al. (2011) doi:10.1029/2010JD014343,) instead?. This uncertainty should be more clearly stated from the beginning in the text (e.g. in the introduction/abstract).

Specific comments:

Section 1

P 22540, L 9 It would be easier to read if the references were complete throughout the paper.

Section 2 is much focussed on the updates respect to the previous works of Wernli and Bourqui. This is certainly necessary, but it would be much better if the present paper was self contained. The method in general should be described with more detail.

P 22541, L 11 The definition of the tropopause used in the paper is not stated in the introduction. Later on it can be deduced that the authors are referring to the 2PVU surface. Since the definition of the tropopause largely affects the numeric value of the fluxes, this is not a minor issue.

P 22541, L 24 More information should be added to make the article self contained. Also, explicit references to sections and or figures of previous works could be provided.

P 22542, section 2.2. The explanation is a little confusing. Please describe the algorithm in general before describing the refinement, e.g. refer to Fig 1 and explain it at the beginning of the paragraph defining regions 3 and 4 in the text.

P 22543 L 15: Why this value of mass? which is the mass of the different regions of the atmosphere assumed? Could you provide some references?

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P 22543 L 18 downward net flux? In a particular region or throughout the stratosphere? Yes, the point is discussed later on, but a hint should be given here, to at least mention that this will be discussed below.

Section 3

P 22544 L 25: Related to the discussion about the convenience of using 2 PVU as limit for the tropopause, in the polar regions the 2 PVU surface is very low and this could have an influence/ bias the results of upward injection.

P 22545 L20 Specify for clarity what is meant by sloping isentropes.

P 22546 L1 define the references properly with name and year.

P 22547 L8 This could be more specific: the Andes extend from close to the Caribbean to the Southern Ocean. You may be referring to the Altiplano (also known as Andean Plateau) in a situation similar to the Tibetan plateau in the Northern hemisphere.

Section 3.1.5: it is indeed remarkable that the warm pool is not at all shown in the PBL to stratosphere plot. The short timescales and the slow ascent present in the meteorological winds used in this study may be part of the explanation for the discrepancy. ECMWF winds represent tropical convection averaged but the 6 hourly average would slow down vertical ascent. The picture could change using global 3-hourly or 1-hourly fields. Inclusion of a convective velocity scheme in the trajectory code itself may show interesting differences (see Pisso et al (2010) doi:10.5194/acp-10-12025-2010, figures 1 and 4) .

Section 3.3 and fig 8 Is S in percentage in the figure?

P22553 L6 The Andean Plateau?

Section 4. Especially for the calculation of tracer fluxes, the definition of tropopause can have an influence and bias the result. The PV tropopause (2PVU - 380 K) is sloping with respect to the thermal (WMO lapse rate) tropopause. An inclined control surface

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may have an impact on the estimated flux, mainly for the overall fluxes. Probably less for the deep exchanges.

P22555 L2 this is just an approximation and this should be clearly stated in the text.

Section 5

Section 5.1 discusses some caveats of the method but misses a very relevant aspect: the dependence of the flux on the choice of the control surface. As stated before, the same calculation performed with the WMO tropopause or other definition is likely to yield different results. It is true that the choice depends on the scientific question and the available technical tools. There may not be a unique answer, but this caveat should be mentioned.

P22560 L22 although not perfectly, convective parametrisations can be included in Lagrangian models (such as in FLEXPART).

Conclusions

P22568 L5 The fact that the 2 PVU definition of the tropopause seems to include portions of the stratosphere (see Berthet et al (2007), fig 2 cited in the manuscript) within the troposphere may have an influence on this strong TST in the polar regions.

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