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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.

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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 PV-threshold used to define the dynamical tropopause. Furthermore, our methodology is now explained in more detail.

The article "A global climatology of stratosphere-troposphere exchange using the ERAinterim 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 PVU380 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 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).

We now mention this issue in the introduction and discuss the sensitivity to the control surface chosen in the new Sect. 5.1.

Specific comments:

Section 1 P 22540, L 9 It would be easier to read if the references were complete throughout the paper.

The references are now 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.

We have re-written and significantly expanded this section. The methodology is now described in 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.*

Of course this has been an unfortunate oversight in the previous version. We now clearly state our choice of tropopause definition in the introduction and discuss the sensitivitiy of our results to an alternative choice (3.5 pvu/380K) in the new Sect. 5.1.

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.

See answer above (we have re-written and significantly expanded this section).

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.

We have re-written this paragraph and now explain all labels in the text (Sect. 2.2).

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?

This formula has now been moved to the methods section. Its origin is the hydrostatic approximation. Given the regular starting grid of our trajectories, each trajectory is representing an air parcel with dimensions dx in the horizontal and dp in the vertical (pressure coordinates). The mass of such a parcel is given by $dm = \rho \cdot dV = \rho \cdot dx \cdot dx \cdot dz$, where we assume the density ρ does not change significantly over the volume dV. Using the hydrostatic approximation ($dp/dz = -\rho \cdot g$), the formula reads $dm = \rho \cdot dx \cdot dx \cdot dp \cdot g/\rho = 1/g \cdot dx \cdot dx \cdot dp$. We now explain the role of the trajectory starting grid in defining the calculation of dm.

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.

We now mention the discussion in Sect. 3.5.

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.

You are absolutely correct. The net upward flux is strongly reduced when choosing 3.5 pvu instead of 2 pvu, suggesting that most TST occurring in the polar regions is vertically shallow. This effect is now described in the new Sect 5.1.

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

We have re-written this sentence: “This is readily explained by quasi-isentropic transport on isentroping sloping downwards towards the equator due to the strong baroclinicity in the extratropics.”

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

We have replaced all abbreviated references by the full versions.

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.

We have specified the latidude range (20S to 30S) in which most deep STT enters the PBL. This is just south of the Altiplano but it would certainly be interesting to study the similarities between this region and the Tibetan plateau.

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) .*

We agree with this explanation and of course, we would very much appreciate a higher temporal resolution of ERA-Interim. The comparison of our methodology with FLEXPART (which uses a parameterised convection scheme) has been performed for case studies by Meloen et al. (2003) and Cristofanelli et al. (2003, both cited in the manuscript) who found that fluxes of a stratospheric tracer calculated using LAGRANTO and FLEXPART agree quite nicely.

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

Yes, the symbol $\%$ denotes 'percentage' or 1/100.

P22553 L6 The Andean Plateau?

The peak over the Andes is located around 30S, which is, to our understanding, too far south of the Andean Plateau to be directly linked to it.

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

We now discuss the flux across the 3.5 pvu surface in more detail in the new Sect. 5.

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

The calculation of the ozone flux is now stated in the methods section and it should now be apparent that this is only an approximation. We have in fact also included the effect of moisture on the density of the air parcel but the influence is negligible such that we prefer to only show the approximative formula.

Section 5

Remark: this is now Sect. 6 due to the newly introduced Sect. 5 (sensitivity studies).

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.

We now clearly mention the sensitivitiy of our results to the choice of tropopause definition. Generally, a 'flux across the WMO tropopause' is not a well-defined concept since a continuous surface is required for the calculation of fluxes across it. In the case of 'multiple tropopauses' (or 'tropopause jumps'), the WMO definition is not continuous in space. Also it is often discontinuous in time leading to artifacts when investigating STE. This problem is not present when using the dynamical tropopause definition. We now show the flux across the 3.5 pvu isosurface, which in the zonal mean corresponds well to the WMO tropopause (see Sect. 5.1 and Figs. 15, S5, S6, S7, S8).

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

A comparison between STE estimates calculated with LAGRANTO and FLEXPART can be found in Meloen et al. (2003) and Cristofanelli et al. (2003, both cited in the manuscript). See also reply to your comment on Section 3.1.5 above.

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.

It is by definition impossible to include portions of the stratosphere within the troposphere if one regards them as different parts of the atmosphere separated by the tropopause (given the latter is a continuous surface spanning the whole globe). Clearly, regions which are marked as 'stratosphere' using one tropopause definition lie within the 'troposphere' using another tropopause definition. In our opinion, no tropopause definition exists that is superior to all others in all aspects, regions and seasons. Indeed, the upward net flux in the polar regions is reduced when using 3.5 pvu instead of 2 pvu (see the new Sect 5.1 and Fig. S9).

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