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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratosphere-troposphere exchange from the Lagrangian perspective: a case study and method sensitivities" *by* M. S. Bourqui

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The author is thankful for the excellent and helpful comments of the two reviewers. A revised manuscript will be prepared according to all relevant reviewers' comments and submitted to ACP.

Please find below my replies to individual comments of Reviewer 2 (G. Vaughan).

General comment from the reviewer: a) "there have been a number of other studies investigating the effects of model resolution; only that of Gray (2003) is referred to here. The conclusion that 1° resolution is the coarsest acceptable resolution to study STE in a model is not new, nor is the fact that the nature, as well as the magnitude of STE is different at 5PVU to that at 2PVU (which is no real surprise after all). Equally, the 'important consequences for chemistry' (i) and (ii) are obvious. The paper needs to

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relate its conclusions more clearly to previous work in this area and draw out what is really novel here."

Reply: The study by Gray (2003) provides the most comparable study discussing the resolution sensitivity of a method estimating STE. I agree that a distinct comparison of my study's parameters sensitivity results with other studies is merely lacking in the manuscript. Such a discussion will be added accordingly in the Introduction and Conclusion of the revised manuscript. The discussion will include resolution and PV level sensitivities.

It is correct that the study by Siegmund et al. (1996) concluded similarly that the 1° horizontal resolution gives results similar to using 0.5° resolution but quite different to using 3.75° resolution. The recent study of Gray (2003) merely confirmed this by showing a larger gap between 1° and 2°resolution than between 0.5° and 1°. However, to my knowledge there is no consensus yet on the coarsest acceptable resolution for STE estimates, and it is necessary to check thoroughly the sensitivities of our STE quantification methods.

It is correct that we can globally expect the mass flux through 2PVU to be larger than through 5PVU. However, most case studies focus on one single PV-level and there is again no consensus on which PV-level is best appropriate for STE estimates. Here, I show that differences exist not only in the overall STE mass fluxes between different PV-levels, but also marked differences exist between the geographical distributions of STE at different PV-levels. In particular, the 4PVU level seems to mark a transition between two different STE regimes. This result needs of course to be confirmed climatologically.

The important consequences for chemistry (i) and (ii) might be obvious theoretically, but the rough mass flux across the tropopause is still often considered as a proxy for the chemical forcing. Here it is suggested that the chemical forcing due to STE might significantly differ from the rough STE mass flux in terms of temporal evolution and

ACPD

4, S1677–S1680, 2004

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geographical distribution.

Part of the Introduction and the Conclusion will be reformulated in the revised version to highlight more precisely the results of the present study and relate them to other studies' results.

General comment from the reviewer: b) "I would like to see more discussion of the nature of TST across 2PVU. Looking at Fig. 4, the overwhelming impression is of far more STT than TST, except for the region around the subtropical jet-stream in (h) which is not referred to at all in the paper. Nevertheless, on p. 8 we are given (for the 4-day period) an overall mass exchange of 763 Gtonnes for STT and 554 Gtonnes for TST - they are in fact very close to one another. This presumably means there is a widespread, small TST everywhere outside the 'streamer'. What is the physics of this? Is is real?"

Reply: The large pattern of TST in Fig. 4 (h) occurs at the boundary of the hindcast simulation domain, and is therefore very likely to be a pure boundary effect. The total mass exchange estimates are made on a sub-domain which does not contain this feature. This point will be stated more clearly in the revised paper.

The local STT mass flux is much larger than the TST mass flux, however the overall STT and TST mass fluxes are relatively close to one another. This suggests that a light widespread TST occurs everywhere. Although the original idea of the manuscript was to focus on the large local mass fluxes, I agree that the significant magnitude of the overall TST flux requires a discussion. Such a discussion will be added in the revised manuscript.

Reply to Minor points: 1) Revised version corrected accordingly. 2) In Wirth and Egger (1999) the trajectories are computed to advect passively the tropopause, and the volume enclosed between the real tropopause and the passively advected tropopause gives a measure of the cross-tropopause mass flux. Our method type (3) is defined as the methods which use a trajectory-based Lagrangian representation of the flow.

ACPD

4, S1677–S1680, 2004

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The trajectory method of Wirth and Egger (1999) is of the type (2) as it estimates explicitly the difference between advective and total part of the tropopause motion. This will be clarified in the revised version. 3-5) Revised version corrected accordingly. 6) Derivation of PV means here the whole process which produces the PV field from the basic fields (u,v,w,t,p). I think the term is correctly used, as far as it is not mixed up with 'differentiation'. 7-11) Revised version corrected accordingly. 12) Yes, revised version corrected accordingly. 13-23) Revised version corrected accordingly. 24) I mean here that the mass flux quantified with a near zero residence time threshold would only depend on the non-conservative sources at the tropopause exactly. But, if the residence time is larger, the probability for an air parcel to stay in the troposphere or the stratosphere after exchange depends on the meteorological conditions in the upper-troposphere or the lower stratosphere. This point will be clarified in the revised version. 25) Changed with 'less explicit'. 26-28) Revised version corrected accordingly. 26) I don't understand the question. It might be a problem to do with the print out of the picture, but I can not see any problem on my version.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 3249, 2004.

ACPD

4, S1677–S1680, 2004

Interactive Comment

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