

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

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 1.

General comment from the reviewer: "My one general concern regarding the usefulness of the results relates to the sensitivity of the cross-tropopause transport to data resolution. The author investigates this by degrading the original data to coarser resolution. In practice, of course, this is not done and what happens is that different studies use models of different resolution. The sensitivity to the use of degraded model data is not necessary related to the sensitivity to the use of a coarser model. This caveat

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should be noted."

Reply: The effect of resolution on a STE quantification method can be investigated following two ways: the repetition of the hindcast simulation at different resolutions or the degradation of the resolution of a hindcast simulation. Both methods have caveats, and it is important to have the caveats in mind when interpreting a specific study. The repetition of simulations at different resolutions introduces, beyond the pure resolution change, a change in the meteorological conditions. This latter change may well depend on the particular setting of the model's parameterizations. In this study, I wanted to make sure to keep the same meteorological conditions and focus on the STE quantification method itself. The underlying motivation is to understand better the effect of the resolution of a data set on this particular STE quantification method. This approach is in particular often used in relationship with operational (re-)analysis data sets. This point will be made clearer in the revised manuscript.

Reply to specific comments: 1) Not changed; the term 'intrinsic' is used in its broad sense. 2) A short justification will be added for the choice of transition time. 3) Technical details will be added accordingly. 4) The reference will be added accordingly. 5) '2 September, 06:00UTC' will be removed. 6) Changed with: '... and has almost disappeared by 5 September, 00:00UTC'. 7) (1) Yes, it is explicit in the text. 7) (2) It is not the purpose here to explain the processes leading to these STE patterns; accurate explanations would require a further analysis of detailed physical mechanisms. The exchange pattern above the cloud is a dipole STT/TST. 8) It is true that some TST exchange events occur around the top of the cloud, however, the density maps (Fig. 4) do not show anomalously large TST fluxes in the region and therefore, at this stage, it would be misleading to associate the green lines seen in Fig. 7 to the cloud top. Some more information on TST will come with the new manuscript version. 9) The statement is based on the two hypotheses: The vertical velocity is larger and spatially much more variable in the troposphere than it is in the stratosphere. In a degraded horizontal resolution, a single value of w is kept as representative of the vertical motion over a larger

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area than in the original resolution. Problems are expected to occur in particular where large spatial variations exist in the vertical wind field. The sentence will be clarified. 10) Assimilated data are believed to introduce significant noise through the vertical velocity due to the assimilation scheme. But, in our case we are using the output of a model and therefore we can hypothesize that the w field at 0.5° resolution should not introduce significant spurious variability. As a consequence, the large number of exchange events associated with short residence times of the ($0.5^\circ, 1h$) line in Fig. 10 is either real or due to the smoothing of the tropopause. However, it is not possible at this stage to distinguish between these two contributions, but the tropopause smoothing is definitely expected to introduce spurious events. These considerations will be made clearer in the new manuscript. 11) These additional experiments were made to minimize the dependence of the results to the length of trajectories. The trajectories performed here are the longest trajectories possible. This justification will be added in the revised version. 12) Changed with 'average'. 13) Yes, this will be mentioned explicitly in the section 4.2. 14) Yes, but to a much smaller extent. The revised version will be corrected accordingly. 15) Point (ii) will be corrected to say that a large dependence on the residence time is seen in STE mass fluxes which varies upon the particular meteorological conditions in the region of the tropopause. As a consequence, the end chemical effect of STE is expected to be influenced by the transport properties in the broader region of the upper-troposphere and lower stratosphere.

Reply to technical comments: 16-31, 34, 36): revised version will be corrected accordingly. 32) The grey shading should give an impression of the structure of the tropopause. I think it does, and the two contours help capturing the important features. 33) The meaning of blue and green at the surface will be added in the caption. The presence of the surface helps the reader to locate the perspective. 35) It is true that there are many lines, but they are clearly distinct in the STT and TST panels at least and give an important information.

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