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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of stratospheric intrusions and intercontinental transport on ozone at Jungfraujoch in 2005: comparison and validation of two Lagrangian approaches" *by* J. Cui et al.

J. Cui et al.

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Replies to the comments of reviewer:

We thank for the valuable comments of reviewer

1) Major comments: In page 1449 at lines 21-23, the authors mention that Ordonez et al. (2005) suggests positive ozone trends in winter due to enhanced flux from the stratosphere. I have not found such a solid conclusion in the cited article. Instead in another more recent paper from the same lead-author (Ordonez et al., GRL, 2007) it is concluded that Lagrangian model simulations indicate that changes in down-

ward transport of ozone from the stratosphere into the troposphere were dominated by changes in lowermost stratospheric ozone concentrations rather than by variations of cross tropopause air mass transport.

A: Thanks for the comments, the reference was wrong, which is corrected in the revised manuscript.

2) In page 1452 at lines 21-23, the authors mention that they have contacted forward simulations from the North American PBL and of stratospheric ozone tracers. I am not quite sure why they selected the forward approach and not the backward approach from JFJ. From my point of view I find more sensible the forward approach since the analysis is for a single station.

A: We agree that the forward mode in FLEXPART simulation is not the optimal set up for our application. We selected forward simulation, because we initially attempted not only to investigate the influence of SI and ICT events on JFJ receptor site, but also to evaluate the geographic variability in the surrounding area of JFJ caused by SI and ICT events. We thought that this information of the surrounding area would be valuable in order to test whether some discrepancies between measurements at the receptor site and model simulations would be attributable to small scale features (being discussed in diploma thesis of Siegrist and Kunz, Intercontinental air pollution transport, stratospheric intrusions and implications for European air quality, ETHZ, Institute for Atmospheric Science and Climate, 2006). Since this information is not the primary purpose, it is not explored in the paper.

3) In page 1452 at line 18, the authors refer to the forcing ECMWF meteorological fields for their calculations. What is the horizontal grid resolution of the forcing field? Is it simply similar to the relevant interpolated fields?

A: Thanks for the comments. The ECMWF fields have a resolution of T511L60, and are then interpolated onto a latitude/longitude grid.

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4) In page 1455 at lines 8-10, the authors state that due to the fact that LAGRANTO does not simulate any diffusion, using trajectory ensemble is also a cost efficient way to qualitatively capture diffusion. I cannot understand if you do not have as physical process diffusion in the calculation how to qualitatively capture diffusion based on trajectory ensemble. Please specify this point.

A: We agree. Displaced trajectories are designed to evaluate the coherent of the air flow. Nevertheless, if the trajectories are not coherent, it might be viewed as qualitative evidence for the enhanced potential for mixing. The revised manuscript has been changed accordingly (see Sec. 2.3).

5) In page 1463 at lines 6-9, the authors mention that the measured NOy concentration during this long SI event was found to be significantly elevated, which might be due to the mixing with uplifted polluted air from the planetary boundary layer, causing increased O3 titration, thus to large extent weakening the stratospheric signature. Additionally it can be also just mixing and dilution of the stratospheric air with PBL air with lower ozone concentration which may cause weakening of the stratospheric signature.

A: We agree with this comment. We changed the text in the revised version of the manuscript (see Sec. 4.3, second paragraph).

6) Minor comments: In page 1449 at line 13, I think the references for previous studies on stratosphere-troposphere exchange events should be in chronological order and not in alphabetical order. Furthermore the reference James et al., 2003b should be James et al. 2003a since it is the first time cited in the manuscript.

A: Revised accordingly.

7) In page 1454 at lines 23-26, the authors state that the model output of CO and O3 was interpolated to the height of JFJ. Was it interpolated to the real height of JFJ or to the model height of JFJ?

A: Revised accordingly. The wording in the original manuscript was confusing. Actually

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we used the grid-cell average directly from FLEXPART output, which contains JFJ site. We changed the wording in the revised manuscript (see Sec. 2.2). The physical height of JFJ was extracted as receptor point from the FLEXPART/LAGRANTO model. This selection was make in order to describe the fact that JFJ often resides in the free troposphere, particularly in winter as found by many field studies, see eg. Zellweger, et al. 2003.

8) In page 1450 at line25, Just for the information of the authors, apart from the study of Stohl et al, 2000 cited here there is also a Stratosphere to Troposphere Transport case study using FLEXPART from the EU-project STACCATO by Zanis et al., ACP, 2003.

A: Thank you for the comment, the reference was added.

9) In page 1456 at line 6, replace "In this terms" with "In these terms";

A: Revised accordingly.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1447, 2009.