Atmospheric Chemistry and Physics Discussions

Interactive Comment

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.

Received and published: 9 April 2009

Replies to referee 1:

We acknowledge the constructive comments of reviewer

1) Setup of the FLEXPART model: I fear FLEXPART was not ideally set up for the purpose of that study. First of all, since the focus is on a single measurement site, I wonder why FLEXPART has been run forward in time. It would have been much more efficient to run it backward in time. Furthermore, results should have been more accurate (and also more straightforward to compare with LAGRANTO), since the simulations could

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have been started at exactly the measurement point, instead of having to use grid-cell averages. I am a little concerned also about the number of particles used in total (below 1 million). Is this really enough to obtain statistically significant results at the receptor site, given the 1x1 degree and relatively high vertical resolution? FLEXPART would allow you to write out associated uncertainties. Have you checked these uncertainties? If the number of particles is too low, you can introduce substantial noise onto the model results, which would degrade model performance. I do not expect the authors to repeat all the simulations but I think they should at least explore and comment on the uncertainties introduced by this setup.

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.

We are to some extent confident that the chosen 1 million particles for ICT simulations are sufficient for the present study because: 1, the chosen number of particles is not randomly distributed but proportional to the actual CO grid concentrations and the transport of the particles shows a very coherent structure. The visualization of case studies generates reasonable plumes, which provide confidence. 2, due to technical limitation (more than 1 million particles created technical problems), we restricted the number of particles.

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Unfortunately, the results of FLEXPART calculations were lost due to a technical problem, and therefore associated uncertainties cannot be obtained without recalculation.

2) The 10-day duration of the FLEXPART simulations and LAGRANTO calculations is definitely on the short side for capturing intercontinental transport events. While it is true that these fast events are the most likely to be seen in the measurement data, especially in summer (when atmospheric transport is slower) you are likely to capture only a minority of events.

A: We agree. In order to get more information, one-year 20 days backward trajectories using ERA Interim data have been calculated with LAGRANTO after receiving the comments. The analyses based upon the newly calculated trajectories show that using 20-day trajectories, we are able to identify twice as many ICT signals from the North American PBL to JFJ, when comparing with 10-day trajectories (25% vs 12% when using reference trajectory plus 4 horizontally 0.5^o displaced trajectories; 26% vs 13% when using reference plus 4 vertically 10 hPa displaced trajectories), which is in accordance to the comments of the reviewer.

We added a short paragraph regarding to comment 2 in the revised manuscript (see Sec. 5, last paragraph).

3) Both FLEXPART and LAGRANTO were using ECMWF input data. The two models are, thus, not really independent of each other. This should be clearly mentioned and commented on.

A: We agree. The argument is now addressed in the conclusion part (See Sec. 6, at the end of second paragraph).

4) Page 1454, line 24: Why were CO and O3 values from FLEXPART interpolated? FLEXPART writes out grid-cell averages (not point values), so you should just take the

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grid cell containing the measurement site.

A: We agree, the wording in the manuscript was confusing. Actually 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).

5) Page 1455, line 9: I wouldn't fully agree that by calculating 5 trajectories, you could say this is a cost efficient way to capture diffusion in trajectory models. Trajectory models just don't have diffusion and this does not change when you calculate more than one trajectory. This rather explores the sensitivity of the results to initial position.

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

6) I am wondering about the 50

A: Unfortunately, we did not understand this comment.

7) In Figures 5 and 6, it is virtually impossible to clearly see whether there is agreement between models and with the measurements. I suggest changing these figures by removing the Lagranto panel. Instead, I would color the background in all the other panels according to identified events: e.g., yellow for LAGRANTO, orange for FLEXPART and red for both. This should more clearly allow seeing whether, e.g., a drop in humidity agrees with events identified by the models.

A: Thank you for the suggestions, Fig. 5 and 6 were changed according the reviewer's suggestion.

8) The Conclusions section is far too long and unstructured. I would shorten it by at least 50%.

A: The conclusion part has been strongly shortened in the revised manuscript.

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Minor languages:

9) Page 1448, last line and at many other places: why not call "sustaining time" simply "duration" of an event?

A: Revised accordingly.

10) Page 1451, line 6: small-scale turbulent and diffusive processes: isn't that just the same? Unless you mean molecular diffusion (which should not be important here), small-scale turbulence is often represented as a diffusive process.

A: Revised accordingly. Replace "small-scale turbulent and diffusive processes" by "small-scale turbulent mixing" in the revised manuscript.

11) Page 1456, line 6: In this terms -> In these terms

A: Revised accordingly.

12) Page 1457, line 4: "situations were undisturbed free troposphere" reads awkward.

A: Revised accordingly.

13) Page 1464, line 11: comparions -> comparisons

A: Revised accordingly.

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