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Interactive Comment

Interactive comment on “Tracing troposphere-to-stratosphere transport above a mid-latitude deep convective system” *by* M. I. Hegglin et al.

Anonymous Referee #1

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General comments:

This paper combines airborne tracer data sampled in an upper level trough adjoining on its eastern side a mid-latitude deep convective systems, Lagrangian diagnoses and modelling results in an attempt to show evidence that deep convection and lightning strokes may have a large impact on unexpectedly high reactive nitrogen (NOy) mixing ratios sampled in the lowermost stratosphere. Neither the ECMWF model nor the mesoscale model CHRM show convective activity sufficiently well developed in the UTLS region to qualitatively justify the enhancement of NOy. The claim of the lightning-origin enhancement is best justified by a rough estimation of NOy enhancement based on the combined mapping of convective influence air mass (backward trajectories com-

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bined with detection of cloud heights in METEOSAT infrared images), registered lightning strokes and typical values for NO-production per lightning stroke. The difficulty of the case study and the high uncertainty range in the made assumptions are emphasized in the paper. An additional interest of the paper is the observation of tropospheric filaments on the western side of the upper level trough whose formation by long range transport is well diagnosed with a potential vorticity reconstruction technique.

The paper is well written, contains sufficient novelty with regards to potential impact of troposphere to stratosphere transports and merit publication. However, possible effects arising from a background problem in the calibration of the NOy instrument should be further investigated, possibly leading to even more careful conclusions of the paper.

My main comment is about the lack of clarity on the evaluation of the precision of the NOy measurements due to the incomplete elimination of side effects from a slight contamination of the NOy analyser with ambient air that has occurred during takeoff of the aircraft. Justifications given to explain that the expected precision of $\pm$ 8% (2σ) was damaged to $\pm$ 50% (2σ) in the first flight hour and to $\pm$ 16% (2σ) for the rest of the flight are not clear, beyond the understanding that there are inherent difficulties to calibrate such a complex instrumentation based on chemiluminescence and on reduction of NOy species to NO. Looking at the comparison between the time series of NOy measurements and NOy values expected from a correlation with ozone (Fig. 2), one could wonder if the time decreasing difference could be partially explained by the decreasing side effects of the initial contamination of the instrument. The existence of such a decreasing contamination effect could relax the constraint authors have to explain why the difference between observed and expected NOy mixing ratios is larger in segment I than in segment II while the probability of enhancement by lightning activity has been shown higher in segment II than in segment I. The authors need to clarify this aspect of the paper.

Specific comments:

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Abstract and elsewhere: Aren't "deep stratospheric intrusion", "streamer", "trough" loosely semantic expressions used throughout the paper to refer to an upper level trough ?

Section 2, page 6: First and second effect. Not clear what authors mean.

Section 4, page 9: Figure 1. Clarify origin : ECMWF or HRM.

Section 5.1, page 13: "This indicates that tropospheric air has been mixed in ...". It is in contradiction with findings based on in-situ measurements and stated on page 11: "The sharp features of the tracers imply that the mixing ... has not been completed yet, ..." and with the presence of isolated maxima of observed H_2O mixing ratio (Fig. 2c). Data do confirm that the tropospheric intrusions are not yet mixed with the environment.

Section 5.1, page 13: "In agreement with this, the tracer correlation has a slope ...". It is not clear that there is agreement because in this sentence "This" referred to the precedent point (see above) claiming that mixing is at work, which may not be the case. Couldn't the slope of the tracer correlation be soiled by the decreasing effect of the initial contamination of the NOy instrument ?

Section 5.1, page 13: "However, investigating the trajectories started from the RDF grid ... (not shown). This may serve as an explanation ..." . Why not ? But, don't the large uncertainties relative to origin of air parcels with such a long period of time (10 days) for backward trajectories sidestep the issue ?

Section 5.3, page 16: There is no conclusion on the inability of the limited-area model to reproduce a convective activity that could be related to deep troposphere to stratosphere transports. Is it sensitive to the choice of the case study or to the convective parameterization or do the authors think that such processes can only be tackled with non-hydrostatic models with explicit representation of the convection ?

Section 5.4, convective influence analysis. The most uniform and semi-circular shape of the convective influence plot on Fig. 9 is clearly originating from the mesoscale

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diffluent outflow circulation aloft and north-west of the storm. However, it behaves little evidence of patchy features that could be expected from the sporadic activity of convection. Isn't it a bit improper to specifically refer on the convective aspect ? The criterion used to attribute convective influence just compares air parcel temperature and brightness temperature of the coinciding satellite pixel, there is no information on the nature (stratiform, convective) of clouds detected. Or, is there more specific treatment of the satellite data in Jeker et al. (2000) that should be mentioned here ?

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