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Atmospheric Chemistry and Physics Discussions

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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.

M. I. Hegglin et al.

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Author response to referee comment #1

We thank the reviewer for his or her comments.

Main comment: The reviewer asks for clarifying the possible effects of the observed background problem in the measurements of the NO_y instrument and to discuss whether a decreasing contamination still unaccounted for could explain the slowly decreasing difference between observed and expected NO_y found in flight segment II (cf. Fig. 2).

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Reply: The evaluation of the overall accuracy of the NO_y-measurement was indeed a major concern to the interpretation of the data. Data quality was much better during the later campaigns but the observations on this flight were so interesting and unusual that we decided to explore the reasons for this in a detailed case study. We are confident that the quality of the measurements is still sufficient to exclude contamination problems from being the reason for most of the unusual observations. However, the referee is absolutely right that any conclusions have to be formulated carefully in view of the instrumental problems on this flight.

We have significantly expanded Sect. 2 (Chemical Measurements) including a better description of the contamination problem, of its likely source, of the additional studies undertaken to examine its significance and temporal behavior, and of the estimate of the uncertainty associated with this problem. The NO_y-measurement in general exhibits, mainly due to uncertainties in the efficiency of the conversion of different species to NO, a relatively low overall accuracy of $\pm16\%$ compared to other trace gas analyzing systems. The error bar indicated as blue shading in Fig. 2 corresponds to a rather conservative assessment of the overall uncertainty which was dominated during the initial phase of the flight by the uncertainty in the background correction. Usually, the background signal is determined by linear interpolation of the signals measured between subsequent zero air calibrations. Due to the contamination an additional not well characterized offset had to be subtracted. As mentioned in the text the enhanced background was found to result primarily from a startup procedure during takeoff using for security reasons ambient air instead of pure O_2 to generate the reaction agent O_3. This startup procedure produced a contamination of the analyzer. Its influence exhibits an exponential decrease and could be reproduced in laboratory studies. By subtracting this contamination signal (modeled as an exponentially decreasing signal) the zero air calibration values were reduced to the normal level observed on other flights. The uncertainty associated with this procedure was conservatively assumed to be 50% of the subtracted signal itself and various tests with other exponential fits showed that corrections larger or smaller than this range result in an anomalous background signal

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level and/or negative NO_y mixing ratios. The error bar (blue shading in Fig. 2) should illustrate that, even when taking into account the highest possible error in the data treatment, the exceptional features found in the measurements do not disappear and therefore remain to be explained.

A slowly varying contamination problem still unaccounted for in our data may indeed partially explain the slowly decreasing difference between measured and expected NO_y values in flight segment II, but not entirely. The authors also would like to stress that the conclusions drawn in the paper do not only rely on the NO_y measurements but are supported by the H_2O data which also were showing higher mixing ratios than normally found in the stratosphere in this flight segment (see panel c in Fig. 2). In order to better illustrate the similar behaviour of H_2O on this flight we have added a new figure (Fig. 3b) showing the unusual correlation between H_2O and O_3.

Unaccounted side effects of the initial contamination of the converter may have affected the unusual negative NO_y-O_3 correlation in segment II to some extent because this correlation is established over a long time period of about one hour during which significant changes in the contamination effect may have occurred. However, it can not explain the unusually steep slope $\Delta NO_y/\Delta O_3$ of the correlation observed during the transitions into and out of the tropospheric filaments encountered in flight segment I. These transitions occurred on a time scale of only 1-2 minutes which is much too short for significant changes in the contamination signal. This fact was already stated on page 12 in Section 5.1 by noting: 'The background signal problem may only affect the absolute NO_y mixing ratios but not the relative changes due to the filaments'. This statement was probably not very clear and has therefore been replaced by the above argumentation. Thus, the large discrepancy between observed and expected NO_y in segment I remains as it can not be explained by the contamination problem. We have expanded the discussion of this discrepancy as detailed in our response to referee #2.

The further suggestions are incorporated into the revised manuscript:

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The meteorological system is probably best described as a 'streamer' which is commonly used to describe an elongated upper level trough, or a part of a larger scale upper level trough. We also used the term 'deep stratospheric intrusion' to highlight the fact that the large meridional excursion of stratospheric air in the streamer was also associated with strong downward motion. We agree that we had switched too often between these terms and are now mainly using the term 'streamer'.

Section 2, page 6: The word 'effect' was misleading and does not appear in the revised version of this section.

Section 4, page 9: first sentence extended to: 'Figure 1 shows the potential vorticitiy distribution on a hybrid model level corresponding to about 230 hPa on 10 November 2001 as analyzed by ECMWF and ...'

Section 5.1, page 13, first comment: The mixing in of tropospheric air described here occurred several days before the formation of the filaments. These are two different things and there is therefore no contradiction. The air inside the filaments indeed had experienced only little mixing with stratospheric air by the time of the measurements. However, as it is now demonstrated more clearly in our revised manuscript, the stratospheric air in between the filaments was not a 'pure' stratospheric air but rather an air mass which was already significantly disturbed by transport from the troposphere several days before the development of the tropospheric filaments. Section 5.1 has been significantly extended and a new figure has been added to clarify this point. See also our response to referee #2.

Section 5.1, page 13, second comment: Please refer to the extended Section 5.1 in

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the revised manuscript and our response to referee #2. The steep slope in $\Delta NO_y/\Delta O_3$ can not be explained by the contamination problem as already mentioned above. A contamination would only shift the entire correlation line of flight segment I up or down on the y-axis in Fig. 3a but would not change the slope.

Section 5.1, page 13, third comment: The figure which was 'not shown' here has now been added (see our previous two comments). The path of an individual trajectory more than 5 days backward may indeed be rather uncertain. However, our conclusion is based on a large number of air parcels in the vicinity of the flight path in segment I which show a rather consistent picture with significant transport from the troposphere into the stratosphere and of air originating in the vicinity of the tropopause deeper into the lowermost stratosphere.

Section 5.3 page 16: The following text will be added to the manuscript on page 16, line 12: *The predictive capability of the CHRM for the location and particular timing of such an observed small-scale tracer anomaly is limited. This might be due to the particular model formulation for subgrid-scale or non-hydrostatic processes - and in fact, it is conceivable, that an element of both is present. For instance, processes associated with deep convection could be poorly represented by the parameterization, and subgrid-scale, non-hydrostatic or small-scale turbulent effects are inaccessible for investigation with the present model's approximations.*

Section 5.4: The criterion used to calculate 'convective influence' indeed only compares air parcel temperature with brightness temperature of the coinciding satellite pixel. The only additional criterion used was that the cloud top temperature must be below -40°C to exclude low level or thin cirrus clouds. However, leaving out this restriction does not change the picture at all. Basically all clouds contributing to the colored domain of convective influence in Fig. 9 (in revised manuscript Fig. 10) were either active deep convective clouds or (anvil) remnants of previous convection. This can

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best be seen in an animation loop of the half-hourly satellite images between 9 and 10 November confirming that the massive cirrus cloud decks present over the western Mediterranean developed out of deep convective storms. A somewhat more patchy distribution is obtained if cloud top temperatures are restricted to temperatures below -50°C in order to better account for active thunderstorms only. The overall picture, however, changes only little using this more restrictive criterion as seen in the updated figure.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 169, 2004.

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